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THE CASE OF JOHN KINSEL. I.

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This case is here presented in the hope that it will be an addition of some value to the small number of cases of like nature which have been already published. For obvious reasons, the principal in the case wishes to have his identity unknown, and hence all possible precautions are taken to that end. To assist in this purpose, the name used here is fictitious, and even those to whom the writer is indebted for help in the preparation of this paper are unnamed, in order that the object may not be defeated. The appropriate portion of this article is to be used in the writer's 'Psychology of Alcoholism,' shortly to be published, and there due acknowledgment will be made in connection with the names of many others to whom he is indebted. The writer wishes, however, to acknowledge here his obligations to the subject, Mr. Kinsel, who recognizing the scientific value of the case, has given his consent to the publication of this article, has furnished all available data, and made some valuable suggestions, assisting in every way possible.

PART I.

The writer wishes to insert another foreword. The presentation of this part is purely descriptive, and carries with it no theory whatever. It is necessary to use certain terms in order to be understood, but these terms are used simply to aid in the description, and do not carry with them any theoretical implications. For example the term 'double personality' does not imply any theory, not even the theory of a double personality,

but is simply descriptive of a state or states, concerning which this is the common term.

John Kinsel was born in one of the most beautiful and healthful country districts in New England. His parents are kind, hospitable and intelligent people, highly respected in the community in which they dwell, and living as would be expected of the better class, well-to-do farmer, residing some distance from the railway or any town. On both sides of the family the diathesis is unfavorable for a sound nervous and mental life, showing insanity, alcoholism and other tendencies to nervous degeneracy. The father is a large land owner, possessing over five hundred acres, not all of which is under cultivation. This has and still does entail considerable responsibility and labor on his part, and yet to-day at sixty-six years of age, he is in good health, active, hardworking, capable of doing his full proportion of work. He was able to give his son a common school education, a high school training, and to assist him in his college course. Like the New England farmer of years ago, he makes every fall from twelve to fifteen barrels of cider, which before spring gets strong and intoxicating. Of this he drinks eight or ten glasses every day, but was probably never intoxicated in his life, and would consider a man weak indeed who would become intoxicated on cider however strong it might be. Beside the regular beverage of cider, the old custom of a jug of brandy for haying time was rigidly adhered to, and frequent indulgence was the rule. While all the male members of the Kinsel family drink, only two carry it to excess, viz., John's uncle and cousin.

His father's sister was insane and died in a retreat. In her youth she was morbidly sensitive, but not until the age of thirty-three did she have the first outbreak of insanity. She at first refused to see anyone, and would do nothing else but read her Bible. From this she recovered without being sent to a retreat, but five years later when she suffered from a second attack, she was admitted to an insane hospital for treatment. The diagnosis was mania, and she was discharged as recovered after six months' residence there. The third outbreak occurred six years after the second, when she was again sent to the hospital for

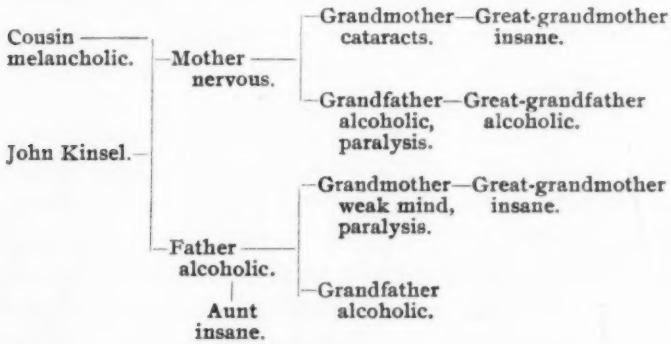
the insane; but between these two attacks there is a history of irritability extending over several years. Upon her admission at this time she was noisy, violent and excited, the form of her trouble again being diagnosed as mania. She remained in this condition about two years, difficult to manage and taking offense at trifling things. She gradually became demented, more quiet and less frequently violent, but retained her irritability which took the form of scolding. She died at the hospital, of typhoid fever, at the age of fifty-nine, without recovering her reason.[1]

John's paternal grandfather drank moderately as do all the Kinsels, but otherwise, as far as can be ascertained, was normal. His wife (John's grandmother) died in a 'fit,' her mind was slightly affected, having had a 'shock' when seventy years of age. One of John's paternal great-grandmothers, his father's mother's mother, died insane; but no particulars concerning her case could be ascertained. In tracing the Kinsel side of the family, we find insanity in two different generations, alcoholism in all the male members, and paralysis.

Turning now to the maternal side af the family we find the record quite as unfavorable. John's mother is neurotic and far from strong. When warm there are noticed urticarious blotches on her throat, probably of nervous origin. Apart from her general nervousness there appears to be no specific trouble. One of John's maternal cousins, his mother's sister's daughter, a young lady of about his age, became quite unsettled mentally when twenty-five years old. She became nervous, ugly, hypochondriacal and pessimistic. She had a special antipathy to her mother, and scolded considerably. She finally refused to work, and resigned a good position as teacher. After three years she completely recovered, and accepted another position as teacher. Mrs. Kinsel's father, John's grandfather, drank heavily all his life, and died of paralysis at the age of seventy-two; but none of his children (John's uncles, aunts or mother) drank at all. John's maternal grandmother was operated on for cataracts of the eyes, after her eightieth year. One great-grandparent, his

[1] The writer is indebted to the superintendent of the hospital for the account of this case. The early records of the hospital being incomplete, they have no history of heredity in her case. The fragmentary history of heredity here presented has been obtained from other sources.

mother's father's father, drank heavily as did all of his children; and one other, his mother's mother's mother, died insane, but there have been no particulars of her case gathered. So, here on the mother's side we find insanity in two generations, alcoholism direct for several generations, excepting the mother, and one death by paralysis. The case of cataract is also interesting, as we find John suffering from the same trouble. We have here on both sides a characteristic epileptic family history. We give it below in outline.



Mr. and Mrs. Kinsel were married at the ages of twenty-eight and twenty-four respectively. A girl was first born to them, but she lived only about twenty-four hours. There have been no other children except John, who was born in 1873, nine years after their marriage. His birth was normal, no instruments being used. He was a nervous child, but healthy and happy, seldom crying. When four years of age, he had a very severe attack of dysentery. He went from one convulsion into another for over twelve hours, and for three weeks afterwards was dangerously ill. The same year while out riding with an old lady, the horse became unmanageable, and running up the side of a steep bank, threw out both occupants of the carriage. The lady fell upon John, the latter striking his head against the edge of a small wooden box. He was unconscious when first picked up, but recovered before long, and immediately inquired for the old lady whom he feared had been killed; he said that he was not hurt except a little on his forehead. Upon examination it was found that the box had come in contact with his

forehead, about a half inch above his eye; the physician who was called said it had made a 'dent' in his forehead. The skin was not broken, but his forehead became much swollen, and all that side of his head, black. He speedily recovered, and apparently with no serious consequences. We might find a traumatic origin for his epileptiform condition here if it were necessary, but his family history would make this superfluous; for not only are we able to charge it to heredity, but given such a heredity we would look for it in his life.

John's life on the farm cannot well be differentiated from that of other children in like circumstances; he assisted about the work and attended the district school. From his earliest recollection he has been nervous. He stuttered badly from the age of four until he was twelve, but gradually outgrew it, so that it was scarcely noticeable when he was in high school. But even yet, if in the company of those who stutter or stammer, it is impossible for him to talk normally. While in college, during a recitation, the professor in charge called upon three students in succession to recite, all of whom stuttered; he then called upon John, who upon trying to recite stuttered so as to be unable to respond, and not until after class when the professor received an explanation of his former habit and present nervousness, was John clear of the censure of both professor and class on the ground of mocking the others. Even now when he talks about his childhood stuttering, he is unable to proceed without perceptible trouble.

During childhood and youth he had very vivid dreams, which continue to the present time. Very early he became somnambulistic; on one occasion when about nine years of age, after having gone to sleep in his bed at home, he awoke to find himself out in the fields. His little dog had followed him, biting at his heels. When he was able to orientate, he discovered that he was a half mile from his home, it being about two o'clock in the morning; he having traversed this distance without accident, and on the journey crossing some very difficult places. He returned to his home and bed. He has always required an excessive amount of water to drink, quite frequently exceeding one gallon in twenty-four hours; he is correspondingly troubled

with polyuria. As a child his father tried to limit him in his drink, and later he himself endeavored to lessen the quantity, but without success. He has since required less, but at the present time drinks more than normal.

All his life, up to the time of the operation upon his eyes, he has been subject to violent headaches, continuing sometimes for days. For a day the headache may not be very severe, then becomes almost distracting. It started over his eyes, then worked back until it apparently crowded the whole head; then sometimes he would become 'light-headed,' and would be finally relieved by an attack of vomiting. These headaches were more frequent and more violent during his high school course than while he was in college; and since the operations for cataracts have taken place, the headaches are much less frequent and severe.

It was decided that John should study for the ministry, and to this end he entered high school to prepare for college. This decision was probably reached, not because of any special religious fitness on John's part, or because of what the older theologians would designate as a call, but for the same reason that so many choose one occupation rather than another, viz., the way was opened more towards the ministry than any other profession, and both the parents and John were ambitious concerning his future, desiring him to enter some profession. Further, the farm life was very distasteful to him, and notwithstanding that he is the only child and must come into possession of this large farm, he still dislikes the occupation of farming.

He entered the high school, and considering the disadvantage of poor eyesight did good work. On account of cataracts, he had not more than two sevenths normal vision in his best eye. The irritation must have been great, nevertheless his health seems to have been good, except for the headaches, and at this time he overcame his stuttering habit. At the age of twenty he entered college, having as a room-mate one of his classmates, who faithfully served him and loyally remained with and assisted him all through his course, even at the expense of a high stand, for which he was ambitious and capable of attainment. It was largely through the help and self-sacri-

fice of this room-mate that John was able to finish his college course; not that John did not work and maintain a fair stand, but his sickness and poor sight were great handicaps to him.

During the freshman year John continued his work like all the other members of the class. He indulged in athletics to some extent, trying for his freshman crew, but did not succeed in making it. He was ill twice during this year; first, from an attack of mumps which was sufficiently severe to affect one testicle seriously for a time, but from this he recovered. After this he had quite a severe attack of la grippe, but after a short illness his recovery was rapid, with no serious consequences. There appears to have been no abnormal mental or nervous trouble during this year, with possibly two exceptions. There were strange swellings of the hands at night, accompanied by neither pain nor inflammation nor disability of any kind. No remedy was used but the swelling went down spontaneously and quickly. This may have been of nervous origin. The other possible exception was the great tendency to sleep, often much of the day being spent in this way. He could sleep any time and anywhere, but he attributed this state to his tired eyes. The summer vacation following the freshman year was spent at his home on the farm, assisting in the haying, and in the autumn he returned to college to take up the work of the sophomore year.

It was in the sophomore year that the somnambulistic states began to show positively. There were four stages in his abnormality, viz., first, asleep, with eyes closed, lying down; second, asleep, with eyes closed, sitting up; third, asleep, with eyes closed, walking around; fourth, asleep, with eyes open, walking around and carrying on the ordinary duties of life. The first of these periods corresponds in time to the sophomore year; the second to the greater part of the junior year; the third to the last part of the junior year and the first half of the senior year, and the fourth to the latter half of the senior year, and for nearly a year following graduation. The term 'sleep' will be used to designate the abnormal state in all its forms, for it was thus named by John and his friends on account of the way in which the state originated, but it is hardly appropriate for the last stage of the abnormality.

At the beginning of these states in the sophomore year, John was sleeping considerably in the daytime as he had done during the freshman year. His class-mates who came into his room noticed that he was suggestible at these times but this was discovered quite by accident. Frequently he would talk when he seemed asleep and when a bunch of keys was shaken near him or a similar noise was made he would start to sing quite lustily, 'Jingle bells, jingle bells, jingle all the day,' etc. The fellows began talking to him and he would answer quite brightly, showing a keener display of wit than when he was normal. Of course his friends did not realize the seriousness of his trouble and it was considered a great joke, furnishing amusement for all his associates. Later another accidental discovery of suggestiblity occurred; some of his friends came into the room and found him asleep; they began singing, in a way common among students, a college song concerning the initiation of freshmen into secret societies. The chorus is rendered loudly and with great spirit, and when this was sung John would arise and beat about the room in a very amusing pretence of initiating the freshmen. It became quite common for those who knew about it to sing this when John was asleep in order to enjoy the sight of the initiation which frequently changed according to the circumstances. It was also noticed at this time that in some ways he was very bright if he talked while 'asleep,' but that when he awoke he failed to remember anything that was said or done while he lay on the couch with his eyes closed, but from his actions and words apparently awake. Most of his companions refused to believe that he was not awake, nor would they accept the statement that he could not remember what had happened, but thought that this was carrying the joke further, such as frequently happens among college boys.

In April of the sophomore year five students including John went out for a sail one Saturday afternoon, intending to return within a few hours. They got becalmed outside the harbor, and when a breeze did spring up they were driven to the opposite shore. It was so dark that they could not see to land, so they anchored as soon as they heard the breakers. Cold and hungry,

they remained in this position all night, and the following morning after landing to obtain something to eat, returned home, reaching the college town Sunday afternoon. Their friends, being much frightened, had given them up for lost, and were about to charter a tug-boat to search for the remains. On the sail home, just before entering the harbor, John lay in the bow and went to 'sleep.' He then began to compose and recite doggerel rhyme as fast as he could talk, greatly to the amusement of the others. He described the different incidents of the trip in his rhyme, and soon afterwards awakened. When next he went to 'sleep' they suggested the trip, to which he responded by reciting this rhyme and adding the incidents which occurred after his awaking on the boat. This became very popular among his friends, and seldom was he found 'asleep' for some time after this without being asked to recite the doggerel. It was found impossible to obtain a copy of the poem as it was first spoken, but through the memory of one of the party, assisted by a process to be explained later, nearly the whole poem was reproduced. It exhibited a quick and spontaneous power of rhyming, together with a change of character quite characteristic of the second state. We see here a young man studying for the ministry, of generally good conversation in his normal state, when abnormal producing low, vulgar rhymes. We present the first four lines to show the general style:

"H—b— A—g— had a scheme, a wild, fantastic, fevered dream;
He thought if westward he should sail, before a strong, propitious gale,
That he would find a wondrous land, where gold lay sparkling in the sand;
Green bank-notes grew on all the trees, and rustled there in every breeze."

During the sophomore year his friends discovered that he could be awakened by running their fingers over his face, but this did not always suffice to keep him awake. At one time his room-mate kept account of the number of times he would awake and go to 'sleep' again during a certain time. The exact results have been forgotten, but it was oftener than once every minute for several minutes in succession. In this year not only were his dreams very vivid, but he began to have serial dreams in his normal sleep. He would dream of some person or thing, and the next night, or for several nights following, begin and

continue the dream where he had concluded it on the preceding night. The most important serial was that of a young lady whom he met in his dreams for several years. She was usually playing the piano and her name was Edith. John told of his dreams to his friends, and thus the name Edith became a common one in conversation and joke. During the latter part of this year, while lying on a couch with his eyes closed, he would talk, joke, smoke and move around on the couch as a waking person, but remember nothing of it when he awoke.

The summer following sophomore year was spent home on the farm assisting in the various duties about the farm, especially in the gathering of the hay. He returned to college in the fall encouraged concerning the prospective year's work, on account of the almost total absence, during the summer, of somnambulism so characteristic of the latter part of the sophomore year. The expectations were not realized, for no sooner did the study and the regular term's work begin than the somnambulism again became prominent. He did not sleep so well at night, and began to walk around the college grounds in his sleep. His room-mate would sometimes miss him and go in search of him. He was found at one of the other dormitories at times, where some of his friends roomed. There he rapped on the windows where he could reach, or threw small stones to the windows of those who roomed in the upper stories. They were awakened and took it good-naturedly at first, thinking it was done for a joke, but finally began to resent the frequent repetition of it. He would generally be reported to his room-mate, who would go after him and bring him home, noticing that when he was walking along or going up stairs he never stumbled. For fear that some mischief would befall him, not from his inability to take care of himself in these sleep-walking experiences, but from some pranks of his friends who did not comprehend his real condition, the plan was conceived of his room-mate's locking the door at night and retaining possession of the key. This was done, and, as far as the room-mate knew, never once did he try to get out; but after two months' success they got lax, the key was left in the door and the sleep-walking again started.

In the sophomore year the abnormal states began when he was lying on a couch with his eyes closed, but at times this was succeeded by his sitting up with his eyes closed and taking an active part in some of the affairs about the room. This began about the time of his returning to college in the fall, the beginning of his junior year. At this same time it was found that rubbing the fingers over his face did not suffice to awaken him, and a new expedient was resorted to, viz., spanking him on the buttocks with a book or some flat and heavy article. This was very successful for a time, and not infrequently John would request his friends to awaken him thus. Sometimes the very threat or posture of spanking him was sufficient to awaken him, and the suggestion that his friends were spanking him was frequently taken and resulted in his awakening. This method was finally ineffectual, and at one time a class-mate spanked him at intervals all the afternoon with a large Latin lexicon, but notwithstanding the physical pain and his shouting and crying, he remained asleep the whole day.

He continued to be very suggestible and dreamed a great deal. During the year while sleeping near a radiator, his friends sprinkled water on his face to try to awaken him. When he awoke he told them of a dream he had had. He dreamed that he was at a fire and was very warm (suggestion from the heat of the radiator) and that the firemen had turned the hose on him (suggestion from the water sprinkled on his face).

In the 'sleeping' state he seemed at times to be much brighter, wittier, and in many respects more intellectual. His friends delighted to find him 'asleep' when they went to his room, on account of the fun in his retorts and conversation. Sometimes he would start up with an exclamation and his friends would carry on a conversation with him from this beginning. One day while sleeping on the couch, with his friends conversing in the room, he suddenly started up saying, "H—l—'s dead and gone to hell." Someone replied, "Tell us about it." He then began and gave a description: "Prexy D——— preached the funeral sermon. He took his text from the second chapter of Colliseums—He hath grinned what he could." This was very appropriate, for H—l—'s smile was a

standing joke among the boys. John then composed and repeated some doggerel as fast as he could talk, commencing: 'H—l—'s dead and gone to hell.'

During this year he showed a wonderful exaltation of memory. The best example is that of remembering several lines of Greek prose while studying with some class-mates. John was reading while the others were finding the unfamiliar words in the lexicon. In the midst of the preparation John went to 'sleep.' When the time came for him to read again, with but a glance at the book, he turned away so that he could no longer see the book, and then repeated six lines of the Greek as though he were reading it, a feat entirely beyond his ability in his normal state; in fact, to but glance at a book and then repeat six lines of Greek prose, would be an accomplishment out of the range of almost any one, especially a student none too familiar with Greek. Some of the classmates attributed this to some telepathic or clairvoyant power, but it was noticed that when he went beyond the sixth line, he still continued with Greek, not according to the text, but he repeated a combination of Greek words that he remembered, the words being put together regardless of the sense. This shows that memory accounted for all, not only that he had no clairvoyant power, but because he did not go beyond the sixth line, that he was not reading. This trait of substituting other words and composition for the original, when memory failed, will be spoken of again when we come to deal with his experience in the hypnotic condition.

During the last part of the junior year the third stage of the somnambulism developed. Before this he had gone to 'sleep' on the couch, with eyes shut, responded to suggestions and talked considerably; next he had sat up with eyes open and participated in certain actions about the room; but now he commenced to wander about the college grounds, with eyes shut, yet without receiving any harm. Probably, as frequently happens in the hypnotic and other somnambulistic states, the eyes were not entirely closed and admitted of some vision. Near the latter part of this year (May, 1896) he had a slight attack of jaundice. He went home for a few days and upon

recovery returned to college. During this time there appeared a bloody sweat upon his forehead, caused evidently by the strain incident to a violent attack of vomiting.

In the spring of the junior year there developed some epileptic attacks. The seizures were only slight, but some were sufficient to throw him to the ground, and during some of them he was unconscious. The first one known of happened after he had come down stairs from his room. The last that he remembered was arriving at the foot of the stairs. When he came to himself, he was in the closet, the place for which he started. He did not know how he got there or what had transpired since he left the foot of the stairs, but from the fact that his lip was cut, his face bruised, and his clothes dirty, he concluded that he had fallen down. When he came to himself he was still dazed and felt peculiar. Shortly after this at the advice of the college physician he went to his home for a few days. Here while harrowing he fell to the ground in an unconscious condition. The third was seen by his room-mate. They were in the room together, John being in his normal condition. He felt the attack coming on and moved over to the couch on which he fell. Immediately there appeared tonic contractions of the extensor muscles, his head was thrown back, his eyes rolled up, his legs, arms and fingers rigidly extended so that it was impossible to bend them. There was no foaming, no blood, no clonic contractions, and he did not hurt himself in the least. His room-mate put water on his forehead and he returned to his normal condition, not having been totally unconscious. There were some other attacks of which we have no definite account, but probably all less severe than these three, and not numbering more than ten or twelve all told. When alone at one time he thought that he felt one coming on, and by resisting it, he considered that he had prevented it. It will be noticed that these came invariably when he was in his normal condition. Besides this 'grand mal,' there were numerous attacks of 'petit mal.'

The summer vacation following the junior year was spent on the farm, as previous summers had been. During the three months, he was 'asleep' not more than two or three times, and then not for very long. In the fall he returned to college to

complete the last year's work. It was during this year that the somnambulism became most aggravated, and showed itself in the most vivid and interesting form. Immediately on his return, the trouble came on again very much as it had been at the end of the junior year. He walked about with his eyes closed, presenting a rather ghostly appearance, yet making his way about without any harm to himself or others. At this time there were some feats performed which some of his friends thought almost superhuman. On one occasion he was lying on the couch in the corner of the room, with his eyes closed while two of his class-mates were playing checkers. The table on which the checker-board was placed was some distance from him, and at least two feet higher than his head, so that normally it would be impossible for him to see the board. He did not appear to be paying any attention to the play, when suddenly he cried, "You can jump two there!" Both those playing and those looking on laughed, thinking that he intended it for a joke; but he got up, went over and showed them where the two men could be jumped, a move which none of those around the board had noticed. Only in one way can this be explained normally. Of course we know by experiment with hypnotic subjects, that when the eyes are apparently closed, there is an opening sufficiently large through which to see; and the fact that John's eyes were apparently closed does not mean that with the hyperæsthesia so common in cases of this kind, he could not see. But according to the normal laws of vision, it was impossible for him to see when the board was at least two feet above his head. John was an excellent checker player and usually took a lively interest in the contest; it is barely possible that being very familiar with the game, he had watched the hands and arms of the players without being able to see the board itself, and thus kept the game in mind before him. No other explanation occurs to the writer without resorting to clairvoyance, for telepathy it was not, as no one knew of the move except John.

On another occasion John played a game of chess with a classmate when his eyes were closed, and in addition to this he was blind-folded. He played through correctly and won the

game. Any attempt on the part of his opponent to move out of turn, or to remove a man from the board was immediately detected. He did not feel all over the board, but put his hand on the man which he wished to move and put it in its appropriate place. One thing that would assist him in detecting moves out of turn was his excellent hearing. This was more acute on account of his poor sight, a feature that we see illustrated in the case of the totally blind. At another time he was tested by his friends after the game. His eyes were closed and different chess-men were placed in various places on the board, some of them being behind a tobacco box. He made mistakes in naming the men until he touched one of them, when he appeared to see the whole board, and rapidly told where the different pieces stood. Another feat frequently performed was the recognition of persons introduced by the wrong name, when his eyes were closed. When they spoke, his acute hearing would account for this, but the hyperæsthesia and slight opening of the eyes would be sufficient explanation of the mystery.

In November of this year he went to the room of a class-mate to be tutored in German. He would not study at first, but finally sat down. The class-mate began to read and continued until he noticed John's book drop from his hand and on looking up discovered his eyes closed. When the reading stopped, immediately John said, "Go on, damn it." The reading continued and as a test, words were omitted or put into improper connections. Whenever this was done John objected and demanded a proper reading. Not long after this his friends asked him to recite, as he frequently did for them when 'asleep.' He chose a reading quite popular among the boys, entitled 'How Ruby played.' This purported to be an account of a rustic who went to hear Rubenstein, the great pianist, play, and is written in an appropriate style. It is an endeavor to show the effect of music upon the emotions. It ends very boisterously as follows:

"—P-r-r-r-rlank! Bang! ! ! lang!
perlang! p-r-r-r-r-r! ! Bang! ! !"

At the last word he jumped off the floor as high as he could, and when he alighted he awoke, looked around at the laughing

fellows, and was utterly at a loss to know what had taken place. The last thing that he remembered was being down stairs about an hour previously. Instead of reciting he would sometimes entertain his friends by imitating the professors in lecturing, bringing in their idiosyncracies with considerable skill.

Before January of his senior year there developed the fourth and last stage of his somnambulism, that of going about in a secondary state with his eyes open, what Binet[1] calls 'vigil-ambulism.' At this time he was very boisterous, and his first appearance to his room-mate with open eyes was quite exciting. John entered his room to find his room-mate there alone. He was 'asleep,' and had evidently been drinking; but told his room-mate that he was going to call on his cousin who was in town for the day. His friend being afraid that he would disgrace himself in his partially intoxicated condition, tried to dissuade him. This he was unable to do, so stood against the door and prevented his exit. John was apparently very angry, threatening all manner of injury, but finding this of no avail, he grasped a large bottle used for holding spring water, and lifting it in the air he started for the door, threatening his room-mate with assault. As he came toward the door with upraised arm and bottle, his room-mate saw his eyes open for the first time when he was 'asleep,' and he looked very wild. The room-mate did not stir, and John gave up the proposed visit.

Another class-mate with whom he was quite intimate, describes the first time that he saw him with open eyes, which was a few days later. While 'asleep,' John had gotten into an altercation with a class-mate across the hall, who had little patience with him. John's room-mate succeeded in getting him into his own room, but John wanted to get out and continue the quarrel, threatening great bodily harm to his opponent. The room-mate, as before, placed his back against the door to prevent his going. John took down from the wall an old revolutionary sword, which was one of the decorations of the room, and ran at his room-mate with the sword, jabbing first on one side, then on the other, the class-mate being almost paralyzed with fear for the safety of the room-mate. John seemed to know that he

[1] 'Alterations of Personality,' p. 3.

was acting a part, so was careful not to strike anyone. Finally the room-mate grasped hold of John, they clinched and fell with the room-mate on top. This seemed to take all the spirit out of John, and he arose whimpering like a child, and saying, "Where is my cigar?" he had lost this in the struggle. He had his eyes open at the time and looked vicious. The excitement seemed to cause his eyes to open in both of these cases, and thereafter they remained open when he was 'asleep.'

This last stage was the fully developed 'double personality,' and now let us give as full a description as possible of this secondary state. In the secondary state John remembered all of his past life, but when he returned to the primary state he could not remember anything that had taken place during the secondary state. His memory of the details of his primary state was frequently more acute in the secondary state than in the primary state. There is only one incident of which we know which shows any memory of the secondary in the primary state; it is as follows: when in the secondary state at one time, John was boisterous, partly on account of his natural temperament when in this condition, and partly through the influence of some alcoholic beverage. He became so violent that he was arrested and taken to the station-house. As soon as his friends heard of his predicament they applied to the authorities, who upon hearing of his infirmity, very kindly and courteously ordered his immediate release. His room-mate took him home and he went to bed and to sleep. He awakened in his normal state, and began to relate a dream which he had — the dream was an exact account of his arrest and the subsequent events. He asked his room-mate if it were so and he was quickly assured that it was not. This is the only trace of any memory of the secondary state by the primary.

Apart from the memory, there is frequently a great difference between the two states, which is shown not a little in the character. Naturally, John is good-natured, kind-hearted, generous, sympathetic, — in short a good fellow and a kind friend. When 'asleep' he became very surly. To some persons he appeared to take a special antipathy in this secondary state. With a few of these it was evidently nothing more than a letting loose

and displaying normal feelings toward them. He would not like them when 'awake,' but of course would not show it; when 'asleep' there was no attempt at control. There were two class-mates in particular with whom he was never pleasant, showing his dislike to them by every means in his power. He would call them by mean and ungentlemanly names, and in his doggerel rhymes would go out of his way to say some unkind thing.

But these were never confined to any particular persons; it was characteristic of him to be surly and disagreeable. Even to his room-mate, to whom he was under such great obligations and whom he normally liked very much, he was usually surly. It was a common thing for him to say that he was going to move out of his room because his room-mate was so 'grouchy'; and he would frequently blame others for being in this condition. John and the writer were always on the best of terms when he was normal, but a few times when 'asleep' he tried to engage in a quarrel. While in his room on one occasion near the end of the senior year, he ordered the writer to leave the room, stalked energetically to the door, opened it and said, "Do you see the door? Well, go out and do it damn quick." His command not being obeyed, he seized a chair and raised it over his head in a threatening attitude, but no attention was paid to him, and before long he lowered it. Finally both went out together, and he continued his abuse and after walking along a short distance, he seized the writer by the neck with one hand and hit him on the mouth with the other, but not hard enough to do any injury. Only on one other occasion did he try to engage the writer in any altercation.

Not only would he endeavor to quarrel with various persons, but he would frequently show his surliness by mean acts. The writer has seen him enter the room of a class-mate where there were three or four persons sitting, walk up to the desk, and with one sweep of his arm knock from the desk on to the floor a whole pile of twelve or fifteen books, then turn around and walk out of the room. The least word frequently sufficed to anger or offend him. With this he was often boisterous, shouting so as to be heard some distance, or again he might be puerile or clownish.

He appeared to have little judgment of the fitness of things; his speech might be nonsensical or improper, and his acts rash and precipitous. Along with his judgment of ordinary things went his ethical judgment. He seemed to be almost entirely different from the normal in this respect, which fact was shown by his language, temper, and his partaking of intoxicating liquors when 'asleep.' The latter is just mentioned here and will be taken up later in detail. He smoked when normal, but smoked most when abnormal. He was careless about money matters when 'asleep,' borrowing indiscriminately and not caring to pay back when 'asleep,' and knowing nothing of the debt he had incurred when he awoke. Some of his classmates thought ill of him on this account, for which of course he was not responsible. Even when he had money of his own he would wake up and learn that he had been intoxicated, yet upon examination find that he had as much or more money than when he went to sleep, and of course he would be unable to tell from whom he had obtained it.

He did not consider the cost, his judgment seemed weak here, and frequently he contracted debts which he would never think of doing if normal. On one occasion he purchased an expensive pipe for which he did not pay. On awaking he enquired concerning it, and when he ascertained the circumstances from his room-mate, returned it to the store. He at another time subscribed for a paper which he had refused to do when awake, and again he contracted with a firm to canvass, but on awaking he was enabled to have the contract annulled. While in his room one day he complained before the writer of being sore and stiff, especially in the arms. His room-mate questioned him when next he went to 'sleep' and found that while 'asleep' he had purchased a snow-shovel, gone a few blocks above the college and shovelled off a sidewalk for which he had received sixty cents. He engaged to come next morning and do further shoveling for one dollar, and left his shovel there during the night. In the morning he was normal, but before noon he went to 'sleep' and asked what time it was; he was told that it was half past ten, and then he related his yesterday's doings, saying that he had lost his job for he had

promised to be there early in the morning to shovel. Although told where the shovel was he would never go and get it.

His will appeared to be weaker, and physically he was weaker when 'asleep.' His room-mate is a slight fellow, and when awake John, who is strong and muscular, could handle him, but when 'asleep' the conditions were reversed. His room-mate also thinks that when John was 'asleep' a slight difference could be detected in the quality of his voice. While his eyes were closed any one could tell when he was in the abnormal condition, and at first when he opened his eyes it was comparatively easy to distinguish the two states. His whole appearance was different and the change in his character showed very plainly. As he came to be more and more in this abnormal state it became correspondingly difficult to determine, and finally no one except his room-mate could tell, and even he at times was unable to detect the difference. It was not only so with others, but with John himself. Frequently the writer has accosted him in the morning with, "Well John, how are you this morning?" to which he would reply, "I'm asleep, been asleep since eight o'clock," or "Woke up asleep," but later on it was difficult at times for him to tell which state he was in. One evening he and his room-mate were sitting before the fire, when the latter said to him, "John, are you awake?" John thought for a moment and then was unable to tell. He was asked if he remembered certain circumstances, and in this way it was found that he was 'asleep,' for memory was the final test. There were certain stock questions, which his room-mate asked him; if he remembered, so that he could answer them, he was 'asleep'; if not, he was awake, for the circumstances occurred when he was 'asleep.' When it was difficult for John to tell his true state he would test himself by these questions, but it is obvious that this would be when he was 'asleep,' for he could not remember the questions when he was awake. He could sometimes tell by the way he felt, and his friends could as often tell by his mood. The latter was not always an index, for the writer has seen him 'asleep' when he was pleasant and apparently perfectly normal as far as mood was concerned, but this was very exceptional. If when 'asleep' he had been very

angry, or in a serious altercation with any one, he would know it when he awoke by the way he felt. He did not know any of the details, however, the cause, the place, or even the person with whom he had quarrelled.

John was liable to go to sleep at any time; there seemed to be no rule about it with this possible exception — he was more disposed to be abnormal after studying hard, or exciting himself; therefore he was more liable to be 'asleep' in the afternoon than in the morning. He would go to 'sleep' between two sentences and continue the conversation so that no one would notice the change. His friends naturally attributed his abnormal states to his eyes, and considered that the strain caused by the eyes upon the nerves was not only the occasion of the individual attack but the total cause of his trouble. They noticed that he was almost always 'asleep' for Hebrew class, which came in the afternoons, and attributed this to the strain on the eyes, caused by deciphering the Hebrew points which he would study in the morning. It was also noticed that if he would take a vacation for a few days or weeks he was almost if not wholly free from attacks, and in the summer vacation only two or three attacks would occur during the four months, and these for not very long at a time; while in college they came to occupy quite half of his time. This supposition was reasonable from the standpoint of a layman, but expert testimony contradicts it. The opinions of two oculists and two eminent neurologists were obtained on this point, and they agree in affirming that the eyes were neither the predisposing nor the exciting cause of the trouble. To explain the 'post hoc' they say that while using his eyes he was also using his brain, and that this was the cause rather than the strain on the eyes; and that when he was home on the farm he not only did not use his brain so much, but that he was also in better general health. The strain of the eyes would have no more effect than the tiring of the arm, or any of the muscles of the body. The cause of the trouble as given by one of the neurologists was the epilepsy, in fact, that the abnormal state was epilepsy, the abnormal state being the equivalent of an epileptic seizure. This accords well with the diathesis and the history of the case. One physician

mentions the possibility of a self-hypnotization, on account of the peculiar nature of the cataracts. When he looked at anything, he tilted his head forward and looked up, as it was found out later, looking over the top of the cataracts. This is a condition favorable to hypnosis, in fact is the Braid method of hypnotizing and a method in common use to-day. This would be valuable if we were at a loss for a cause; the epilepsy is so evident that this explanation is superfluous, as a *predisposing* cause, but it will be referred to later as the exciting cause.

Besides the liability of his going to 'sleep' at any time, he was as uncertain about waking up. The length of time in these states ranged all the way from less than a minute to several days. Above, we spoke of his waking and 'sleeping' several times in less than as many minutes. This was the shortest time. The longest time was during the latter part of his senior year. He went to 'sleep' on Wednesday morning at half-past nine, and that afternoon he, the writer and several other students went to a symphony concert. For the rest of the week he continued to go about his work as usual, attending classes and performing his regular duties, and in the middle of a sermon Sunday morning he awoke at a quarter of twelve. This makes his longest known 'sleep' four days and two hours. When he would awake he was unable to tell how long he had been 'asleep,' and had to look at his watch to find out the time of day, and find out in some way, usually by asking his friends, what day it was, in order to carry on his work.

In hiding his ignorance of time and other circumstances he has become very skillful. This he has tried to cultivate on account of his desire to keep the knowledge of his condition from the public. By adroit questioning he is able to discover the time and circumstances without betraying his ignorance of them. He has told the writer of his greatest predicament of this kind. John acted as a waiter in a students' club, a custom quite common among students trying to contribute to their expenses. He awoke at one time when coming from the kitchen with his hands and arms piled with filled dishes. He had not the least idea where any of them belonged, but very skillfully found the right places without betraying his confusion. Many incidents

of this kind taught him to hide successfully the embarrassment caused thereby.

There were different depths to his "sleep," as shown by his different moods, but concerning this there was no rule either. During the latter part of his senior year all efforts to awaken him on the part of his friends were futile; when he awoke it was spontaneously. When 'asleep' one day John threatened someone, and two fellows seized him, threw him down, and pounded him some time in an endeavor to awaken him. He shouted and screamed, but with all their efforts they were unable to succeed, and had to allow him to go. On one occasion, though, he was awakened by a blow. He went to the door of a class-mate's room and demanded admittance; this was denied him. He then broke open the door, grasped an alarm-clock near by and threw it at the class-mate. Then he came over, sat on the knee of the class-mate and demanded twenty-five cents. On not receiving it he tried to obtain it by force. The class-mate could not stand any more, and being considerably the smaller, he struck John as hard as he could on the solar plexus. John immediately awoke, saw where he was, and on turning around perceived a number of fellows in the doorway, who were attracted there by the noise of the encounter. When he realized his position he was so chagrined that he went down to his room crying like a child. This episode occurred with one of his class-mates against whom he entertained a great dislike when 'asleep,' but, as with many others, if he awoke two minutes later he would be on the best terms and very agreeable.

He suffered considerable inconvenience from his trouble as one could imagine, the chief cause being the lack of memory of the events which occurred in the abnormal state. When we consider that during the last part of the senior year, fully one half of his time was spent in the abnormal state, we can see what a handicap it was. For instance he might prepare a lesson while 'asleep' (as he frequently did), and when the time came for recitation he would be awake and unable to remember a word of it. He took notes which were entirely foreign to him in his normal state, borrowed money and made purchases of which he knew nothing, and made engagements which he did

not keep. One day he said to the writer, "Well, I must go over and make arrangements with old D. (Prof. D.) for an exam." He had already made an appointment for the examination of which the writer knew. John continued talking about it for some time until the writer felt sure that he knew nothing of the arrangements, when he was told concerning them. He accepted the matter as settled on account of his knowledge of like situations, and said, "Well then, I guess I'll get to work at it; I haven't much time."

His examinations were difficult for two reasons, first, because probably half of the class work would be prepared and recited when 'asleep,' and he always had to reckon on being awake when the examinations were taken; and second, he could not well prepare for them when he was 'asleep'; for if awake during the examination the preparation would be of no use to him. If he could plan on being 'asleep' during examinations, well and good, he could remember all, and probably remember it better when 'asleep,' for in this abnormal condition he was the better man, not only because he remembered more of his life, but because in many ways he was quicker, keener and more intelligent, and his memory for the events of normal life was better than when awake. The writer remembers going into his room one day when there was to be an examination in sight translation in Hebrew, in which both were interested. John, as usual, on Hebrew days, was 'asleep' and recited from memory the first two chapters of Genesis without a mistake. He read Hebrew much better at sight when 'asleep,' but this was no doubt because he was 'asleep,' so much on Hebrew days that few words were familiar to the normal self. He passed one examination in Biblical literature when 'asleep,' and got along very well with it. In talking with him recently, he said that because he had most of his Hebrew when he was 'asleep,' he had requested the writer to hypnotize him in order that he might the better pass the examination. This was done, and he passed it quite successfully. Of this the writer remembers nothing, but would rather trust John's memory than his own in regard to it.

Notwithstanding the obvious advantages of being 'asleep,' he never liked to be in this condition. Whenever he would tell

the writer that he was 'asleep,' it was always in a voice showing disgust and disapprobation. He did not feel the responsibility for his work, and complained of making many mistakes, and got into more awkward positions. Where he thought that there was any possibility of success he made requests to have his friends try to awaken him. The only time that he ever courted 'sleep' was when he wanted to pass an examination.

The young lady John so frequently met in his dreams was named Edith, and when he had a fiancee his friends transferred the name Edith to her, this of course not being her real name. She was entirely ignorant of John's trouble and was purposely kept so, and quite frequently it was difficult to make explanations. When 'asleep' he would write her certain things, and on receiving an answer when awake, he would not know to what things she referred. When he first had this trouble he would, when awake, find a letter addressed to him which had been opened, and would accuse others of opening his letters and reading them, but he soon became accustomed to this situation and jokingly said: "I have double pleasure out of my letters; I get them when asleep, read them and enjoy them, and then when I wake up I read them and enjoy them again."

But more serious trouble was in store. One day when awake he started to see his fiancee, who lived about seventy miles from the college town. He went to 'sleep' on the train and awoke in a depot. He had no idea where he was, although he remembered starting in the morning. He went out and read the name on the depot and found out that he was still en route, being in a depot where he had to change trains, and was awaiting the second train. He arrived at his destination all right, remained awake until the second day, when he went out in the woods with the small brother of his fiancee, to teach him to snare rabbits. There he went to 'sleep,' and returning to the house endeavored to embrace his sweetheart in the presence of other members of the family. This she resented, and on returning to college the regular letter from her did not appear. He was awake and did not know the cause, but suspecting that something was wrong he requested his room-mate to question him when next he went to 'sleep.' This was done, the trouble

ascertained, apologies sent, and peace restored. One incident which caused some surprise at the time was apparently of telepathic nature. One evening while 'asleep' he told his roommate that he knew what his fiancee was doing, and thereon related an account of her going to some social affair and the incidents which occurred there. The next day he received a letter confirming all that he had said. Although at the time some known connection, such as reference to the event in some previous letters was looked for, none such could be discovered.

In the latter part of the senior year John met a graduate student, Mr. X,[1] who endeavored to help him through the agency of hypnotism. John was not very well pleased to have his case examined, for this was what he feared someone would try to do, and naturally he did not wish his infirmity to become a public affair. However, his desire to be awakened overcame his fear of publicity, and he submitted. The writer quotes from Mr. X's thesis:

"On account of the grave nature of the case, I have refrained from disturbing (John) by experiments or from using hypnotism except as a therapeutic agent. Up to date (John) has been placed in slight hypnosis by me three times. The first occasion was an attempt to teach him to wake himself from the secondary state so as to escape the rough handling which was being resorted to. I will describe his visit to my room and the method employed. (John) had never formerly been hypnotized and knew practically nothing about the subject. In actuality autohypnosis and somnambulism had often taken place. The boys had been having him perform all sorts of feats and he was really a very suggestible person. I base what I now write on notes taken at the time.

"March 30, 1897. — About 9 P. M. (John) and his room-

[1] Mr. X wrote a fragmentary account of the case and presented it as an appendix to a doctor's thesis to one of the large universities. He confined himself, however, almost entirely to the events of the latter part of the senior year. Some of the incidents related here were also in his account, but the writer was in no way indebted to Mr. X for them. In fact some of these incidents were obtained from the writer by Mr. X, and the remainder were known by the writer or obtained from independent sources. The work of Mr. X should not be minimized, as it was extremely important, and the account of his hypnotic treatment is copied verbatim from his thesis.

mate called on me. (John) was 'asleep' and his room-mate said they had come over to get me to try my method of waking him. This morning in conversation with (John) I told him I was going to teach him to wake himself. I did not say I was going to hypnotize him; he did not know what to expect; but the sequel showed that he was very susceptible to suggestion.

"We sat for a few minutes talking and eating bananas. * * * After a few minutes I asked (John) if he was ready for me to try my method of waking him. He replied that he was. Up to this time he had sat rather quietly in my arm-chair but speaking heartily when he took part in the conversation. I noticed nothing in his demeanor specially characteristic of a secondary state. * * *

"Without telling him or his room-mate what I was doing, I had him settle comfortably in his chair and asked him to look steadily at a small square of white paper (about 1 in. sq.) which I pinned on my breast. I was standing directly before him and the spot was about a foot and one half from his eyes in an easy position. No attempt was made to fatigue his eyes but only to fix his attention a little. My orders were about as follows:

"'Look steadily at this white spot until your eyes feel drowsy, then close them. Think of nothing else but become drowsy and sleepy. You will have no headache or pain but go right to sleep.' After about a minute his eyes closed. At the same time I held my watch to his ear and continued: 'Listen only to this watch and my voice. They will help you to go to sleep. Go quietly and soundly to sleep now.' At about this stage I asked him if he were asleep. He seemed partly confused by the question, slightly opened his eyes and said, 'I guess so.' Questioning him some days later, he told me it seemed as if the world got dim and my voice sounded far away when I was having him go to sleep. To make sure that he had reached a suggestible state, I held the watch to his ear a little longer and continued, 'Listen to the watch and go to sleep. Go soundly and easily to sleep now, and then do what I tell you.'

"All this had not taken more than three or four minutes. (John's) head was slightly drooping forward and, with closed

eyes, he seemed quite somnolent. Judging him ready for suggestions, I spoke to him in an easy, but firm, voice about as follows: 'I am now going to have you wake yourself up. I am going to tell you to count "One, Two, Three," and then clap your hands together. You understand now it is to count out loud to yourself, then clap your hands sharply together, and at that you will wake. You see I am teaching you to wake yourself. All ready now; count "One, Two, Three," clap your hands and wake up.'

"(John) had remained quiet, though apparently attentive, until I gave the last command and paused. Then he at once counted in an energetic voice 'One, Two, Three,' clapped his hands sharply together, then opened his eyes and looked about at his room-mate and myself in a surprised way. He probably would have betrayed more confusion had he not already become used to waking in unexpected places. As it was he only murmured something like, 'It has worked, has it?'— probably echoing his room-mate's exclamation, 'Well that worked fine!' To break a somewhat awkward silence, I reached to (John) a plate having on it a banana and the skins of those we had been eating previously, saying, 'Won't you have another banana?' This was an unfortunate remark in one way, but brought to light how complete was his lapse of memory. He looked puzzled and hesitatingly said, 'One of those (skins) is mine?'— and stopped with this conjecture, rising inflection, waiting for me to corroborate it. I told him he had eaten one banana while 'asleep' a few minutes before. He said he now remembered nothing since before 6:30 P. M. (Earlier in the day he had been asleep from one to six P. M., at the end of which time he had been awakened in the room of one of his acquaintances; then had stayed awake perhaps half an hour; then had been 'asleep' until this visit to me at about nine P. M.) I explained to him that I had taught him to wake himself; that he did not now remember how it was done, but that he would remember all the next time he was 'asleep;' and that he must then wake himself; I told his room-mate in his presence not to tell him how he was waked. After a few minutes both went home apparently highly pleased.

"After the above I did not see (John) for two days; then I called at his room. He told me that he had found out how I waked him. This came about in a peculiar and amusing way. Going to 'sleep' in his usual off-and-on manner, he had tried my recipe several times with great success and much to his pleasure in thus having some control over his states. He found it seemingly harder to work, however, and resorted to trying to go sounder to sleep before using the formula. While over at his eating club on the morning of April 1, he waked himself up and was surprised to find himself repeating 'One, Two, Three,' and feeling as if he had clapped his hands. After his breakfast he came to his room and stayed awake until about 10 A. M. Was 'asleep' five or ten minutes when he went down stairs. While there he woke himself up by the recipe, but repeated '1, 2, 3; 1, 2, 3; 1, 2, 3;' and clapped his hands several times after he was awake and could not stop it. (John) soon went to 'sleep' again and had a slight quarrel with (a friend). He remained 'asleep' only a few minutes when he woke himself again by seating himself in a rocker, swaying to and fro until he went sounder to sleep, then saying '1, 2, 3,' and clapping. He again had to repeat and finally broke off with the interjection, 'O damn, can't I stop this!' Telling me of his experience later, he said it seemed as if his whole body was paralyzed for a few minutes, except his tongue and hands, which kept repeating '1, 2, 3,' and clapping.

"After my visit of April 1st (John) did not go to 'sleep' until April 3d. This was staying awake more than usual and it is possible he took as a 'suggestion' my merely telling him that I not only was going to stop his repeating, but should keep him from going to 'sleep' at all. I had laughed at what he had told me and assured him that it could soon be stopped. I felt certain that it was a special case of the tendency to continue induced actions which is found so peculiarly in some subjects. * * *

"Monday, April 5, 1897. About 10 A. M. (John) and his room-mate called at my room. (John) was 'asleep' and wished to wake up. He had gone to 'sleep' Saturday afternoon and had been 'asleep' and awake several times since. He had waked himself by my recipe, but had to repeat '1, 2, 3, clap'

too many times. He thought he had repeated it fifteen times; his room-mate had seen him repeat about eight times. * * * When I got ready to hypnotize him he kept on talking and would not fix his eyes as I directed. I left him alone a few moments till he became quiet, then I placed him in a slight hypnosis as on the first day, only cutting the time shorter. I gave him suggestions about as follows:

"I am going to wake you myself this time. Then you must stay awake for the rest of the week. Do not go to 'sleep' when you see A, or anyone. If you should accidentally go to 'sleep' come to me to be waked up. Now when I say '1, 2, 3,' and clap my hands, you will become wide awake and stay awake for a whole week. Have you understood everything? Nod your head if you have. (He immediately nodded.) All ready now: 'One, Two (I think he woke at the word 'Two,' at least he then opened his eyes), Three, Clap'—clapped my hands. He aroused more, looked around puzzled and said, 'Well, I came up to your room again.' I watched for any tendency on his part to 'repeat' as if he had waked himself, but there seemed none. * * * His pipe was lying on the table where he had put it a few moments before. He looked at it and said, 'Have you a pipe just like this?' He thought it was his pipe but did not remember putting it there. As he arose from the chair his back twitched and he started to tell me about having hurt it in the gymnasium. He seemed to have no idea that he had explained all this fully a few minutes before.

"After (John) awoke, I threw the little square of paper at which he had been looking in my open fire. He saw the action and I remarked, 'There goes what we did it with.' He immediately asked, 'Did you have me sign a contract?'—thus unconsciously illustrating how complete was his lapse of memory, and how he was trying to piece it out with conjecture.

"Friday, April 9, 1897.—(His room-mate) saw me in the Library at 11 A. M. and told me (John) had just gone to 'sleep.' He seemed to linger in a sort of half-sleep a few minutes before going fully over. Soon (John) said he had gone fully to 'sleep.' I immediately went over to see him and found him in the room above. He was trying to hypnotize Z. in the manner in which I

treated him. He was not having good success with it, however, and there was considerable fun over it. (John) had remained awake the prescribed length of time lacking one day to finish out the week. It seemed, however, that he had irritable spells which ordinarily went with his 'sleep,' but at the time he would declare that he was not 'asleep.'

"After a little conversation I took him over to (the college physician's) office. In the presence of the doctor I placed him in a slight hypnosis and gave him the following suggestions: 'I am going to have you do three things. 1. You will wake up and stay awake. 2. You will not fight or quarrel with any of the boys. 3. You will wake yourself by counting "1, 2, 3, clap," but not repeat it at all.' I repeated these suggestions to him and had him nod that he understood them. At my saying that he would not fight with the boys he smiled. When all was ready I had him wake himself. He did so in a moderate manner and did not in the least repeat * * *.

"After getting waked up in (the doctor's) office April 9, (John) stayed awake thirteen days. On the morning of April 22 he went to 'sleep' on the train on his way back to college. (He having gone home on his Easter vacation on April 14.) * * * During the week after his return to college (John) went to 'sleep' twice."

Besides the above records taken from the thesis of Mr. X, John was awakened hypnotically three other times; once again by Mr. X in the presence of the writer, once by the writer, and once by the college physician. On May 3, the writer met him on the college grounds, and was told that he was 'asleep.' The suggestion was immediately made that he go to Mr. X to be wakened. He at first refused and began to speak of Mr. X in a very disrespectful and antagonistic manner. After considerable persuasion and argument, and the writer's agreeing to accompany him, he consented to go. But after getting started he did not seem to be real anxious to continue his journey, for he stopped as frequently as he could find any pretext for so doing, and in one instance making a pretext by going into a store and purchasing some maple-sugar. This was partially eaten on the remainder of our way.

At length we arrived at the house and found Mr. X at home. After being seated Mr. X said, "Well how is everybody?" John jokingly replied, "Cutten has gone to sleep." Mr. X then asked how long 'Cutten' had been asleep, to which he replied, "I can't tell very well, but I think since sometime this morning." John offered Mr. X some maple-sugar and they talked for some time, when finally the latter said, "I suppose the best thing for Cutten is to get waked up, isn't it?" to which John replied, "That is what I came up for, but I do not believe your method will work. I tried to wake myself by saying your '1, 2, 3,' twenty times but it would not work." An appropriate answer was given and John was told to lie back in his chair. This he did and after some difficulty, for John would persist in talking, he went off to sleep. The following suggestions were then given: "Now I am going to wake you up in a new way. It is a simple, easy way, but just as good as any. You will snap the fingers of your right hand and then you will wake up. All right, snap your fingers and wake up." At once he obeyed the commands, and looking up to Mr. X he said, "I came up to your room, did I?" then looking around and seeing the writer he said, "Hello Cutten, are you here? Did you bring me up?" He then started to help himself to the maple-sugar, but quickly excused himself for what he considered a breach of manners due to his confusion. The writer then explained that he had bought it on the way up and therefore it was his to do what he liked with, and then he passed it around to Mr. X and the writer. The writer and John then left Mr. X and went to John's room, he asking on the way the circumstances attending his going to Mr. X.

Three days later again the writer met John on the college grounds, and as before, on enquiry was told that he was 'asleep.' John was again advised to go to Mr. X to be awakened. The resistance was stronger than at the other time, very uncomplimentary remarks were made concerning Mr. X, and in addition to this he said that he thought Mr. X had power over him. He resisted every effort to this end, and finally said, "You come and wake me up Cut., you can do it as well as that little pimp." His request was finally complied with and

we went to his room. He was told to look at a thimble held before his eyes and to listen to the ticking of a watch held to his ear. He soon went to sleep and he was told to say 'Presto,' clap his hands together and wake up. This he did with great vigor and awoke. He looked up and said in a pleased tone, "Did you wake me up, Cut.? Bully work." Later in the day he was 'asleep' and ordered the writer out of the room as recorded above.

One evening after the class in physiology John followed the college physician into his office and told him he was 'asleep' and then requested that he be awakened. The doctor put him in a slight hypnosis and told him to slap his knee when he heard the doctor count up to three and awake. This he did and said that he knew how he had been awakened, his knee tingled so that he thought he must have slapped it pretty hard. Besides being awakened these six times his room-mate awakened him once or twice.

On the seventh day of May John went to his home and remained for the rest of the month. During this time he had very few attacks and returned to college on the first of June to take his examinations, feeling comparatively well. From then until commencement he was 'asleep' a number of times, but not so frequently as before his visit home. One thing which has not been recorded and which was quite severe at the latter part of the senior year, as well as all through that year, was the frequent bleeding of the nose. This he has been more or less troubled with all his life.

(*To be concluded.*)

THE DISTRIBUTION OF ATTENTION. II.

BY DOCTOR J. P. HYLAN.

9. The Conscious Period.

From the conditions pertaining to the processes of the subconscious period, we have no reason to think that the number of the elements of an impression are here interfered with by any of the difficulties that might attach to simultaneity. Thus, if we take a passing glimpse of the page of a newspaper the words and letters do not appear to interfere with each other's distinctness, although many of them serve as stimuli at the same time. But we do not perceive these as so many objects with distinct meanings. Thus we have two distinct functions involved. One has to do with the mediating of stimuli in their right number and proportions so far as they are brought within range of the sense organ. The other has to do with the meaning and interpretation of these. Plainly, if some objects are perceived in smaller numbers or less readily than others when all are of equal distinctness, the difference is due to the function of perceiving rather than to that of simple transmission; and such a result would indicate an interference in perception, or, in other words, a lack in an adequately divided attention.

Wundt distinguishes no difference in the number of letters, figures or lines which can be seen from a tachistoscopic exposure, and refers to Cattell's work for details.[1] Cattell used an exposure of 0.01 sec. Twelve cards with vertical lines 2 mm. apart were shown five times each. There were from four to fifteen lines on each, and the subject was told to estimate the number seen. In a similar way letters and figures were exposed, and also one-syllabled words, and sentences. The observer was required to give the arrangement of the single elements of an impression, and different characters were sharply

[1] Wundt, *Phys. Psych.*, II., 287, 288. Cattell, *Phil. Stud.*, III., 121 ff. See especially page 126.

distinguished. The results were arranged according to the numbers of right and wrong cases. If all the objects on the card were rightly judged, it was a right case; if not all rightly judged, then a wrong case. Two was the smallest number of words shown at once, three the smallest number of figures and letters, and four the smallest number of lines. By combining the essential results from the seven subjects, all of whom served in the four kinds of Cattell's experiments with lines, figures, letters and words, and retabulating for purposes of comparison, we have the following table based upon two of his.[1] I have arranged it in four parts corresponding to the four experiments. The column to the left gives the number of objects on the card exposed. Under each of the headings, 'Lines,' 'Figures,'

TABLE VI.

Objects Exposed.	Lines.			Figures.			Letters.			Words.		
	Right.	Wrong	%	Right.	Wrong	%	Right.	Wrong	%	Right.	Wrong	%
2										48	32	67
3				65	5	8	61	9	14	14	46	329
4	50	5	10	52	18	35	50	30	60	19	21	111
5	41	14	34	68	42	62	36	64	178	1	19	1900
6	41	14	34	31	59	190	14	46	329			

'Letters,' 'Words,' are three columns. The first column gives the sum of cases from all the subjects in which the objects were seen correctly, — the right cases. The second column gives the sum of cases from all the subjects in which a mistake was made in seeing the objects, — the wrong cases. The third column gives the per cent. that the second sums are of the first. With some experiments, especially in the cases of the words and letters, some of the subjects dropped out who could not see the larger number of objects correctly, so only those who could see most continued to the higher numbers. This accounts for a comparatively small number of wrong cases and low per cents., *e. g.*, 111 under 'Words.'

This table shows a very marked and constant increase in the number of wrong cases as we pass from the lines to the words. Thus for four objects the per cent. amounts to 10 per cent. for

[1] *Loc. cit.*, pp. 124, 126.

lines, 35 per cent. for figures, 60 per cent. for letters and 111 per cent. for words; while the wrong cases were 67 per cent. of the right cases where but two words were given. The figures, it was noted, tended to form numbers and this helped to increase the number seen over the letters. One could see three times as many letters when they formed words as when this was not the case. If the words formed a sentence, twice as many could be got as when they stood together without this relation. Sentences were also exposed and the length of the sentence which could be read was from three to seven words, depending upon the subject. It was found that the sentence was perceived as a whole. If the sentence were not perceived, neither were the words that composed it; but if it were, the single words appeared very distinct.

These results show two things very clearly. First, that the greater the function of perception that is involved, the greater is the mutual interference and the smaller the number of objects seen; and second, that the close perceptive relations of the objects causes a mutual reënforcement and an increase of the number seen. I believe also that the number of objects which Cattell's table gives as seen is too large, since the cards with letters and figures seem to have been used a great many times, and this could not but have promoted a familiarity with them which would cause them to be partially remembered from time to time.

This close study of Cattell's tables was suggested by what at first appeared as an incongruity between his results and those of the following experiments of my own. As parts of two different series of experiments to be described later, one consisted of exposing simple objects and the other of letters upon cards with the tachistoscope above described. The objects consisted of circles, squares, diamonds, oblongs, etc., all about the same size, of black paper upon white cardboard, with ten objects on each card, and irregularly arranged over the surface exposed. The full width of the opening in the shutter, 6.3 cm., was used, thus giving an exposure which, from beginning to end, lasted 42σ. This gave a uniform exposure to all parts of the field. First the right, then the

whole, then the left of the field were shown successively, but the rate of movement was too rapid to make succession perceptible. The heads of the subjects were placed in a rest one meter from the apparatus; the eyes were fixated upon the fixation mark in the center of the field to be exposed, and the attention distributed over the field so far as possible. After the exposure, the subject gave the number of objects distinctly seen and was then immediately asked to identify them by referring to the card which was given for inspection. There were twenty of these cards, each shown five times, at the rate of twice a week, on Mondays and Wednesdays. The experiment thus consisted of a hundred exposures for each subject, of whom there were six. Table VII. gives the sum of the objects thus

TABLE VII.

Subject.	D.	H.	Mea.	Mer.	R.	Rog.	Average.
Number of objects.	955	791	699	695	619	454	702
Subject.			A.	S.	R.	Y.	Average.
Number of letters.			299	228	238	287	263

seen in the hundred exposures for each subject.

Under the same conditions the black letters previously described, of about the same distinctness as the objects in the last experiment, also irregularly arranged upon white cardboard, were exposed for the same length of time. Here there were but four subjects, and all different from those who saw the simple objects except one, *R*. This fact deprives the table of much of its value, except so far as we may regard the results of these two groups of subjects as generally characteristic. All the subjects were students of more or less training in laboratory work.

Here more than twice as many objects were seen as letters; a result in general accord with that of Cattell, although differences of method make a close comparison impossible. The objects which could not be perceived formed an indistinct blur at the instant of exposure; while those which were, appeared with varying degrees of distinctness and were counted, in most cases, one at a time, from the fading mental after-image which followed the exposure. All remarked a tendency to see the

objects in rows and groups, a device which seemed to assist the getting of a large number, and sometimes the positions were slightly changed, apparently to assist this grouping. This is evidently of a kind with the seeing of more figures and letters when they formed numbers and words, and illustrates the mutual reënforcement arising from close perceptive relations. The experiences with seeing the letters were not essentially different, and are not to be distinguished from those already described with the successive exposure of letters. We have now to ask what bearing does the seeing of a small number of complex objects as compared with that of simple ones, and this reënforcing tendency in perception, have upon the question of the simultaneous distribution of attention.

It has been pointed out that a perceived object may be regarded as composed of more or less discrete elements, as of form, position, color, etc. The elements of form in a circle or square, *e. g.*, are less in number than those composing a letter; and those of a letter less than those composing a word. It has also been noted that what I have called the mental after-image fades very quickly. Immediately after the exposure the objects are distinctly in mind, and may be more vivid even than at the instant of exposure. This, however, is not an ocular after-image but a mental one, and fades out in from one to five seconds, depending upon the subject and conditions; after which, if an object has not been perceived, it cannot be recalled. Now, if we should find that it takes a longer time to perceive an object in proportion to its complexity, we should have an explanation of the fewer complex objects that can be got from an exposure; for since the mind apparently passes from one object to another in succession, and the mental after-image is of limited duration, the number of objects got would depend upon the rapidity with which one's mind could pass from one to another.

In finding the time required to recognize one letter out of twenty-six possible ones, Cattell, whose simple reaction time was 146 σ, reacted in 326 σ, while one short English word from twenty-six took 360 σ, a short German word 367 σ, and a long English word 375 σ. This indicates a distinct difference of time for recognizing letters and words, although much less than

the difference of complexity between them, and shows the effect of practice in recognizing words as a whole. Another series of experiments by Friedrich with three subjects, in which from one to six place numbers were recognized, showed a similar increase. Here the simple reaction time was 186 σ. In passing from the recognition of one-to-two or of two- to three-place numbers, the increase in time was slight. Thus for one-place numbers it was 318 σ, and for three-place, 397 σ. But from here on the increase grew with the increase of the number of numerals in the following steps: 53 σ, 147 σ, 322 σ.[1]

This fact of the increased time of perception in proportion to the complexity of the object seen is an explanation of the fewer complex objects than simple ones that can be seen from a tachistoscopic exposure. But this explanation rests upon the supposition that the mind passes, as appeared often to be the case as examined by introspection, from one object to another as they remained in the after-image. If this transition from one object to another takes place, no simultaneous distribution of the attention necessarily takes place, and tachistoscopic experiments are satisfactorily explained without its use. It therefore came to be my purpose to test this question of mental succession by other than simply introspective means.

We may naturally ask why it should take longer to see a complex object than a simple one. We can see how this would be if there yet remains in the perception of the complex object a degree of succession in its parts. The slow process in learning to read suggests that this is the case with words and numbers. This is supported by the introspective results of recent research in the Leipzig Laboratory.[2] In a study of reading by means of an improved form of tachistoscope, Zeitler concludes that the reading of a word is a successive process. This was shown the most clearly by putting a false letter in the middle of a word. When this was done, the attention would hang there and not get farther. The subject also distinctly noticed intervals in the appearance of the signs of the word in con-

[1] See Jastrow, 'The Time-Relations of Mental Phenomena,' p. 38, and table pp. 32–33.

[2] 'Tachistoskopische Untersuchungen über das Lesen,' by Julius Zeitler, *Phil. Stud.*, B. 16 (1900), S. 380.

sciousness. These signs were formed by (*a*) the arrangement of the controlling groups of letters from left to right, and (*b*) the different vertical height of letters in the structure of the word. Succession was more noticeable with foreigners than with Germans, since with the latter the more rapid assimilation made the process more frequently appear simultaneous.

Numerical results bearing upon this point would be much more valuable. If we can arrange simple objects together in such a way that they do not form wholes with such close perceptive relations as letters and figures possess when they form words and numbers, we might, according to this hypothesis, expect it to take distinctly longer for perceiving a number of such objects than for one, and in proportion to the number presented together, even though the elements composing the groups be very simple. And this result would be weighty evidence against the simultaneous perception of the objects. The tachistoscope above described was used according to this plan. Four white cards were used, all having one small black circle, and in a different part of the field exposed for each. Four cards were used with two such circles, but arranged in different relative positions in each. In a similar way there were four with three circles, four with four circles, and four with five. All the circles on one card were of the same size, so that they might be seen with equal distinctness; but the size was different, varying from seven to eleven mm. in diameter, for each of the four cards composing a set. This was to keep the subjects from reacting immediately to the amount of color rather than to the number of objects; since if the circles were all of the same size, it had previously been ascertained, one would distinguish the number simply from the amount of black upon the card. The electrical connections of the tachistoscope were so arranged that the hands of a Hipp chronoscope were started at the instant of complete exposure of the card, and stopped by the subject's reaction. The shutter of the tachistoscope moved at the same rate as before, and the whole opening was used. The subjects used a five-finger key, pressing the key corresponding to the number of objects seen. The subjects were given a short period of practice twice a week for six weeks before the reaction-time was regularly re-

corded, in order to establish quick connections between the number of objects seen and the response of the appropriate finger. During this practice a different set of cards was used than that when the reactions were recorded, in order to prevent a familiarity which would cause the objects to be seen simply as groups rather than individually. The same hour of the day was always used for each subject. In conducting the experiment, the cards were used in an irregular order so that the subject did not know beforehand to which he was to react. The order of events at an exposure was as follows: the chronoscope was started, the experimenter counted 'one' two seconds before the exposure, and 'two' one second before it. The exposure was made, the subject reacted, and then gave the number of objects seen. It was found of advantage to begin each sitting with a few unrecorded reactions in order to overcome the subject's inertia. The dark room and electric light were used to secure constancy of illumination.

It may be seen that this experiment presupposes the subject's ability to see five simple objects at one exposure, a larger number than could be seen of the letters under the same conditions. During the preliminary training, one subject, *Rog.*, had difficulty in getting five objects. This occasioned the experiment with simple objects, the results of which are given in Table VII. This showed that the average number seen for this subject was about four and a half for each exposure. The record of the five days in which the set of twenty cards was shown to this subject in that experiment shows the sums of the objects seen each day as follows: 83, 77, 86, 97, 111. No doubt a part of this increase was due to a growing familiarity with the cards, but in the reaction experiment which followed, practice had had sufficient influence to prevent any difficulty in seeing five objects.

The choice-reaction to from one to five objects evidently involves the following factors: (1) the perception of the number of objects to which reaction was required; (2) the afferent and efferent processes involved also in simple reaction; (3) the choice by which the number of objects seen was associated with its appropriate motor expression, and (4) the specific time of each finger. While we are especially concerned with the first

of these, all helped to determine the time of the reaction. In order to determine the value of each of these, two other series of reactions were taken with the one already described. One of these consisted of reactions with the appropriate finger when the subject knew the number of objects about to appear. Since the subject was required to perceive before the response only the group as a whole, not the number of objects exposed, the time was that of simple reaction, including the specific time of each finger separately. All reactions belonged to the sensorial type. This series consisted of ten reactions for each finger of each subject. Half of these immediately preceded, and half immediately succeeded the other series.

The other supplementary series immediately followed the main experiment, and consisted of choice-reactions to the five Arabic figures, 1 to 5, presented upon white cards. The figures were of jet black gummed paper and 13 mm. high. Ten reactions with each finger of each subject were also taken here. The difference between the time here and that of the simple reaction is obviously the amount needed for the clear discrimination of the figures and the choice of movement. This difference, regarding the time needed for the discrimination of the different figures as equal, would vary with the quickness with which each figure could be associated with the appropriate finger for reaction. The difference between this series and that of choice reactions to from one to five objects is the difference between perceiving figures and numbers of objects. We may assume that it takes about the same time to perceive the figures 1 to 5 with the possible exception of the figure 1 on account of its simplicity. But two subjects were able subjectively to distinguish a difference of time: to *Mea.* it seemed that the time was slightly shorter for 1, while for *R.* it seemed distinctly shorter, and also slightly shorter for 2 than for 3, 4 or 5. By subtracting the time required for reacting to the figures from that required for reacting to the objects, we get the time needed for perceiving the objects more than that needed for perceiving the figures.

Table VIII. is arranged to allow a comparison of the two supplementary series. The mean variations and averages in

TABLE VIII.

Subj.	Finger Used.	1st.		2d.		3d.		4th.		5th.	
		Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M.V.	Av.	M.V.
D.	1–5 figures.	466	23	529	67	560	58	542	29	502	47
	Simple reactions.	145	22	149	17	129	27	162	37	212	16
	Difference.	321	1	380	50	431	31	380	—8	290	31
H.	1–5 figures.	459	67	484	48	443	45	470	35	531	44
	Simple reactions.	134	8	143	16	162	38	138	15	140	19
	Difference.	325	59	341	32	281	7	332	20	391	25
Mea.	1–5 figures.	485	41	516	21	590	36	604	101	508	31
	Simple reactions.	178	21	153	18	166	34	147	18	147	17
	Difference.	307	20	363	3	424	2	457	83	361	14
Mer.	1–5 figures.	416	36	482	29	494	59	543	48	454	45
	Simple reactions.	130	11	128	15	113	8	144	22	124	10
	Difference.	286	25	354	14	381	51	399	26	330	35
Rog.	1–5 figures.	358	25	345	27	364	39	394	29	398	56
	Simple reactions.	106	5	103	8	97	3	93	8	96	10
	Difference.	252	20	242	19	267	36	301	21	302	46
R.	1–5 figures.	537	30	669	42	687	57	775	53	682	71
	Simple reactions.	204	49	205	47	181	23	165	14	169	17
	Difference.	333	—19	464	—5	506	34	610	39	513	54
Sums of differences.		1824	106	2144	113	2290	161	2479	181	2187	205
Av. of differences.		304	18	357	19	382	27	413	30	365	34
Sums, simple reactions.		897	116	881	121	848	133	849	114	888	89
Av. of simple reactions.		150	19	147	20	141	22	142	19	145	15

thousandths of seconds of ten reaction times are given, while the difference between the series is also given for each finger in the case of each subject.[1] The averages of simple reactions at the bottom show that the first finger — the thumb — was slowest, and the third or middle finger was the quickest. Also the fifth or little finger was most constant, as shown by the mean variation. No strict uniformity, however, obtains between the different subjects in these respects. The averages of differences show either that the figure 1 was more quickly recognized than the other figures, or that it was more readily associated with the movement of the thumb than the other figures were with their respective fingers. Regarding the latter as partially causing this small number, we see that the association time gradually increases until it reaches its climax with the fourth finger; while the mean variation increases in a similarly gradual way, reaching its climax with the fifth finger.

Table IX. presents a comparison between the choice-reaction with figures 1 to 5, the results of which are also given in Table

[1] With one subject, Rog., the averages in the simple reaction series are of five instead of ten.

TABLE IX.

Subj.	Finger Used.	1st.		2d.		3d.		4th.		5th.	
		Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M.V.
D.	1–5 objects.	481	58	531	44	578	48	651	116	537	61
	1–5 figures.	466	23	529	67	560	58	542	29	502	47
	Difference.	15	35	2	—23	18	—10	109	87	35	14
H.	1–5 objects.	511	45	511	51	541	59	577	69	563	96
	1–5 figures.	459	67	484	48	443	45	470	35	531	44
	Difference.	52	—22	27	3	98	14	107	34	32	52
Mea.	1–5 objects.	483	52	550	43	630	63	609	62	516	56
	1–5 figures.	485	41	516	21	590	36	604	101	508	31
	Difference.	—2	11	34	22	40	27	5	—39	8	25
Mer.	1–5 objects.	481	47	542	65	574	75	606	87	495	45
	1–5 figures.	416	36	482	29	494	59	543	48	454	45
	Difference.	65	11	60	36	80	16	63	39	41	0
Rog.	1–5 objects.	415	35	424	39	410	47	454	39	467	63
	1–5 figures.	358	25	345	27	364	39	394	29	398	56
	Difference.	57	10	79	12	46	8	60	10	69	7
R.	1–5 objects.	545	82	698	96	719	112	850	178	664	104
	1–5 figures.	537	30	669	42	687	57	775	53	682	71
	Difference.	8	52	29	54	32	55	75	125	—18	33
Sums of difference.		195	97	231	104	314	110	419	256	167	131
Av. of difference.		33	16	39	17	52	18	70	43	28	22

VIII., and the choice reaction with the simple objects, one to five in number. With the latter, as there were four cards for each number of objects, and as these were shown five times, the numbers in the table are averages of twenty reaction times. The averages of differences for each number of objects in the last line of this table are what especially concern us, as they give the pure perception time of the objects over that of the figures. We see here that the time increases gradually from one object to four with a very decided dropping off for five. The smallest increase is 6σ, that of two objects over one. This probably should be larger, since the tendency for the figure 1 to be recognized more quickly than the other figures would make the difference time for recognizing one object (33σ) too large. It should be observed that the mean variation increases in degrees similar to those for the time of perception, which is additional evidence of the increasing number of successive steps of the mental process as the number of objects increases. This also falls off with the time of perception in the case of five objects. This decrease of time needed for perceiving the five-object cards appears out of harmony with the time for the other cards

and indicates a difference of method for perceiving them. We are able, however, to explain the decrease of time for the five-object exposures. There can be no question that the mental process here involved was different in degree or in kind from that for the two-, three- or four-object exposures, and this difference, as shown by the low mean variation, was fairly constant. The introspective records throw light upon this point. In general, the cards did not seem to present discrete

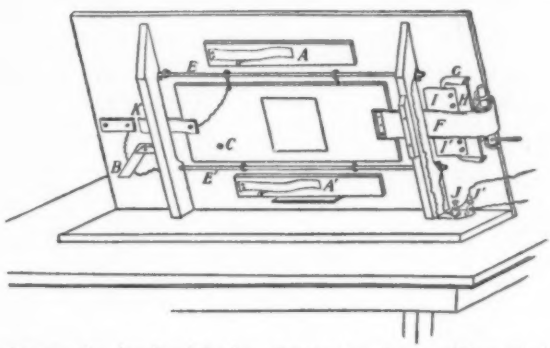


FIG. 1. Shows the back with the shutter in the position giving complete exposure. *A*, *A'* is the card-holder, and *B* the lever, the inner end of which fits against the screw-head *C* when the shutter is drawn back preparatory to an exposure. The shutter slides upon the brass rods *E*, *E'* and is propelled by the rubber-band *F*. *G* is a stuffed rubber tube which deadens the compact of stopping against the stop *H*; while *I*, *I'* are brass springs which fit closely upon the end of the shutter and prevent a rebound. Electrical connections are supplied via the binding-posts *J*, *J'* so that the current is broken at *K* when the shutter has reached the position of complete exposure.

objects, but a figure which, by its general appearance, was assigned to a certain finger and according to its degree of complexity. Counting was often used as a test of this complexity and usually before reacting. On account of the large numbers, the cards with five objects were more difficult to count than any of the others and recourse was had to devices for shortening the process. One of these consisted of telescoping the process in counting, so that one or two of the numbers dropped out and the others slipped over with varying stress, *e. g.*, 'one, three, four-five,' with emphasis upon the last. The grouping of the five objects into one group of one or two and another of three or four was more generally used; while two subjects were able

to recognize the five-object cards immediately, because they were instantly seen as more complex than the four and it was known there were none with six.

Do these methods accomplish a distribution of attention for the five objects? Evidently the last does not, since the group was judged as a single complex object simply having one or more features different from those of other groups. The dividing of the objects into two groups, the constituents of which could be more immediately perceived, is apparently another form of the same thing where the groups were immediately perceived from their form and then combined. Introspectively, this seemed to be a slow method. Probably telescoping was only a device for hastening the process of counting. It was mentioned by but one subject. Table IX. shows two subjects with whom the difference-time was longer for five than for four objects. These were the only ones who did not resort largely to some of these devices for the five objects. *Mea.* observed none, while with *Rog.* the five objects required the most distinct counting.

The question arose as to whether the immediate perception of the five objects as experienced by some was really due to the fact that five was known to be the maximum number, or perhaps to a real distribution especially facilitated by the number five. Would the same result appear for the maximum if it were *e. g.* six? Also was not the perception time influenced by the association time for the different fingers since the main features of the curves for both would coincide? To test this the above experiment was repeated, but both objects and figures ranging from one to six were used. Two five-finger keys were employed, one of which was used by the thumb and first and second fingers of the right hand, and the other by the same members of the left hand. There were four subjects, only one of whom took part in the above experiment. The time for perceiving the objects above that for perceiving the figures was ascertained. Two of the subjects perceived the six objects immediately as more than five. For them the time was distinctly less than for four objects, and for one of these it was also less than for five objects. With the other two subjects the

six objects were not perceived in this way but were counted and sometimes grouped. With these the time was greatly lengthened. The subjects averaged together as follows: for one object 0 σ, for two — 10 σ, for three 16 σ, for four 14 σ, for five 34 σ, for six 133 σ. This result is in accord with that of the earlier experiment although a different proportion of the two kinds of subjects with regard to their perception of the maximum groups would make the form of a resulting curve somewhat different. The evidence against distribution is thus strengthened since this additional experiment supports the interpretation placed upon the former. Whatever may have been the cause of the — 10 σ for the group of two objects, this could not be regarded as a case of distribution since the time is less than that for the single objects.

The argument against distribution in these experiments may be summarized as follows: In Cattell's experiments, as well as with my own, it was found that the more complex the objects exposed by the tachistoscope, the smaller was the number that could be retained from the exposure. Since introspection shows that there is a definite limit to the time during which the objects may be retained before perception is completed, this difference in the number of simple as compared with complex objects that can be perceived may be explained if a longer time is required to perceive a complex than a simple object. Cattell has shown this to be the case with letters and words, and Friederich with numbers. The most natural explanation of this lengthened time for the complex object is that its parts become conscious successively rather than simultaneously, a solution supported by the introspective results of Zeitler in reading words from short exposures, and also by the process experienced in learning to read, where the letters of a word are first recognized one at a time, and then recognized apparently as wholes as a result of practice. If this is the true explanation, we might expect a distinct difference of time to be required for discriminating groups of objects in proportion to their complexity, even when their elements are much simpler than the letters and figures that compose words and numbers, and when no habit in succession, as in reading, has been formed. Our experiment has shown this to be the

case when the elements are perceived as such. When they are not so perceived, evidently the question of distribution is to that extent not involved, since the group, not the element of the group, is the unit for the attention, in the same way that we recognize a word from its configuration rather than by perceiving the letters as such that compose it. We have thus obtained adequate numerical evidence to support the introspective finding that the mental process involved in the tachistoscopic exposure employs successive rather than simultaneous acts of attention.

We have noted the reënforcing effect in the number of letters and figures seen when they form words and numbers respectively. This is evidently caused by previous familiarity with the words and numbers thus formed. In the last experiment this familiarity tended to make the groups perceived as wholes from their configuration rather than to present discrete elements. The introduction of a new set of cards caused hesitation and a feeling of strangeness. The effect of this acquaintance was naturally to vitiate the experiment. It assisted counting. Also, if distinctly remembered, the configuration of the card suggested the reaction rather than the number of discrete objects, and hence caused a decreased reaction time due not to a distributed attention but to the perceiving of the group as a whole. The fact that in spite of this tendency there yet remains a distinct increase of time in passing from the one-object to the four-object cards makes the argument against distribution still stronger. On account of this tendency, the practice set was different from that used in the final series. When the series was over, each subject was asked to note such cards as were recognized after an interval equal to that between the different sittings. An average of 53 per cent. was remembered, although it was difficult to pronounce definitely in regard to many of these. Those remembered were distributed fairly evenly between the different numbers of objects, and were cards which gave both unusually long and unusually short reactions.

10. Reactions to Disparate Impressions.

If it takes a separate associative process requiring an appreciable amount of time to perceive each object in a group when

these objects are of the same kind, we may expect this time element to be increased when instead of similar we deal with disparate impressions. This result is suggested by the experiment in which disparate impressions were counted and where the absence of distribution was shown more distinctly than with the counting of similar impressions. On the other hand, if distribution is possible, it should be in evidence as much in the perception of groups of disparate as of similar impressions.

To test this, the following apparatus was constructed. A wooden screen 58 cm. by 30 cm. was attached to a base so that when placed upon a table it rested in a vertical position with the long axis horizontal. Near the center of the bottom of this were two round openings arranged vertically 1 cm. in diameter and 2.5 cm. apart, with a fixation mark half way between. One of these was filled with red colored glass and the other with white ground glass. Behind these was an oblong shutter hung vertically by a pivot at the top and allowed to swing in pendulum fashion past the two openings in the screen. Two openings were made in the shutter to match those in the screen, and each could be closed by a light-proof valve so that when a light was placed behind the screen, either one, two, or no light impression could be given through the screen openings when the shutter was made to swing past them. Electrical attachments were arranged so that a Hipp-chronoscope was started when the light impressions were given, and stopped by the reaction of the subject. Also at the time at which the chronoscope started a spark from an induction coil could be given to the wrist of the hand to react, by means of dry electrodes. A fourth stimulation could be given by an electric sounder also controlled by the electrical attachments of the shutter, while a fifth, a tactual, was given mechanically. This consisted of drawing a knotted cord through the left hand of the subject, which held it lightly but firmly. The setting of a trigger attached at one end of the cord stretched a heavy rubber band attached to the other end. The movement of the shutter, if desired, released the trigger and allowed the contraction of the band to give the stimulus, by drawing the knotted cord smartly through the hand.

By means of this apparatus any one or any combination of impressions from one to five could be given simultaneously to the subject; these being (1) a stimulus of red light, (2) a stimulus of white light, (3) a click from the electric sounder, (4) an

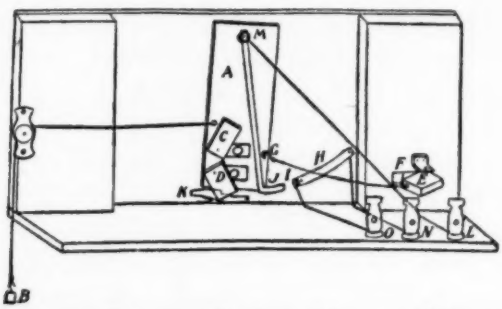


FIG. 2. This gives a diagrammatic view of the rear of the screen. An electric light, suitably shaded, was fixed in the middle. The shutter *A* is made to swing at a constant rate by means of the weight *B*. The valves *C* and *D*, which are carried by the shutter, are drawn back so that in the present position light is allowed to pass through both the oblong openings of the shutter and the round glass windows in the screen. *E* is the trigger for giving the tactual stimulus. When drawn back to the right the cord, which passes through the opening *F* and under tension from the rubber band to which it is attached, keeps *E* in a state of equilibrium, but which is destroyed when the shutter moves, provided the attachment is made at *G*. This allows the cord to pass through *F* about an inch and also through the subject's hand on the other side. *H* and *I* are brass plates over which the brass spring *J* passes to make electrical connections. When the shutter is drawn back and secured by the spring catch *K*, a current flows from the binding-post *L* by way of *M* to *N*. The chronoscope was placed in this circuit. Both the circuit for the electric shock and that for the sounder went through *L* to *O*. *J* pressed flatly against *H* and *I* over a space about 1 cm. long, so that the current was made through *I* before it was broken through *H*. This was to allow time for the overcoming of the inertia of the sounder before the chronoscope started. The light stimuli and also the tactual were given when the chronoscope started, but the electric shock was not given until contact was broken between *I* and *J*. This served to delay this stimulus slightly in order to make the reaction to it more clearly coincide with the others, which would otherwise be prevented on account of its well-known rapidity.

electric stimulus applied to the wrist of the reacting hand, and (5) a tactual stimulus applied to the other hand. The screen was placed 70 cm. from the subject. The dark room was used. As a test of the degree of simultaneity with which the impressions were given, a series of ten simple sensorial reactions to

each of the stimuli was taken from each of the five subjects employed in the experiment. In the following table *S* stands for the sound stimulus; *T*, for the tactual; *R*, for that of the red light; *E*, for the electric stimulus; and *W*, for the white light. The numbers are averages of ten trials from each of the different subjects and are for thousandths of seconds. From the final averages we see that the longest time, that for sound, was but

TABLE X.

Subject.	*S*	*T*	*R*	*E*	*W*
D.	182	169	195	170	167
H.	190	167	165	156	153
Mea.	167	187	177	163	155
Rog.	159	157	143	163	155
R.	204	169	188	182	197
Average.	180	170	174	167	165

15σ more than for the shortest, that for the white light. So far as possible it was tried to overcome, by the structure of the apparatus, the difference of time incident to the use of the different senses, so that when several stimuli were given the time for reaction would be coincident for all.

As there were five different stimuli which could be given and combined as desired in the reactions to these disparate stimuli, we may see that there were five alternatives for the single impressions, ten possible combinations for the double impressions, ten for the triple impressions, five for the quadruple, and one for the quintuple. All the possible variations for each of the five series were used an equal number of times. The five-finger key was used as in the reactions to the groups of from one to five similar objects, and the same subjects were employed, with the exception of *Mea*. Each, therefore, had had a long experience in choice-reactions. The same number of hours per week and the same days were also used here. Since practice seemed an important element of the problem, two hundred reactions were taken from each subject, instead of one hundred as in the experiment with the similar objects. This made forty for each in each of the five series. All five series were kept along together to avoid different degrees of practice for each, and also to pre-

vent the subject from knowing at any time to how many impressions he was to react. In general, the same methods and precautions were used here as in the experiment with similar objects. It will be remembered that one subject in that experiment had difficulty at first in combining all the objects for making correct reactions. In the present experiment with disparate impressions, all of the subjects had this difficulty, and this made necessary a period of practice extending over three weeks before all could react with precision to the groups of four or five impressions, although reactions to the smaller numbers were possible from the first. Even after records began to be taken, a large number of errors was constantly made. During the second week but about one third of the reactions could be recorded, but marked improvement had taken place for most of the subjects by the third week, although even much later than this *Rog.*, who had difficulty before, would have days when not more than a fifth of his reactions were made correctly. Frequently on stormy days, or when a storm threatened, nearly all of the subjects would relapse to an early stage of practice, as shown by the frequent errors. These difficulties made the work proceed so slowly that while it took but three weeks to get one hundred reactions from each subject in the last experiment, here it took eleven weeks to get two hundred.

In addition to errors of discrimination and of choosing the right finger for reaction, which are more or less common to all choice-reactions, those which were peculiar to this experiment were of two kinds. One was caused by the inability to retain the larger number of stimuli in the mental after-image at once. Two or three could be held with comparative ease until perception took place, but the fourth or fifth would sometimes crowd out one already grasped, and this would make an error in the number retained and reacted to. The other kind of error arose from the distinct succession experienced in the maturing of the stimuli in consciousness, which was much more marked than with stimuli of the same kind. The reaction would sometimes be given before sufficient time for this maturing had elapsed, and would hence be for the wrong number. The order in which the impressions matured was not fixed. If for any

accidental reason one or more were unusually strong, they would tend to come in first; and if weak, would come last. The habit in the order when all were given was also likely to change from time to time, but the most common order was: first and second, the visual stimuli at nearly the same time; third, the electric; fourth, the tactual, and fifth the auditory stimulus. The averages in Table X. suggest that this order was influenced by the real order of precedence in receiving the stimuli, the two visual stimuli naturally holding together, when all were given, on account of their related degree of similarity. The subjects, however, did not tend to ascribe it to this.

In preparing for the reaction the subjects were not successful in holding the different stimuli by anticipation in mind at the same time, although it was found that rehearsing them serially distinctly assisted in apprehending them and reacting promptly after they were given. When first given there was a tendency to dwell on the qualities of the impressions, then the process consisted of giving a name and counting them as they matured, before reacting. At a very early stage as the experiment progressed conscious counting disappeared for five impressions, although it would return occasionally. In place of counting, the number appeared as a definite total and reaction took place to a feeling of 'all' or 'many.' No doubt this was assisted by the fact that this number always had the same character and could be easily remembered, whereas the other multiple, as also the single impressions, had five or ten forms in which they appeared. For the combination of four impressions a method of subtraction came after a time to be used, in which the total would be arrived at by subtracting the lacking impression from the total remembered when all five had been given. Here the remembering of the quintuple impression and the subtraction of [illegible] from it would be a shorter mental process than the counting [illegible]our impressions in succession. Both of these methods de[illegible]ased the time, especially that used for the five. Subject R. also used subtraction to an extent for the triple impression. This is a comment upon a feature of his record which is not shared by the others, viz., that the average time of perceiving the triple impression for the second hundred reactions was less than that for perceiving the double one.

Table XI. gives the result of the experiment. The first line of numbers with each subject gives the averages and mean variations of the first twenty reactions, in thousandths of seconds, for each group, single, double, triple, etc., or finger which reacted to it. The second line gives the corresponding numbers for the second twenty for each finger. The third line gives the average time and mean variation for choice-reaction to the Arabic figures 1 to 5.

TABLE XI.

Subj.	Finger Used.	1st.		2d.		3d.		4th.		5th.	
		Av.	M. V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
	1–5 disp. imp.										
	1st 100	796	195	1006	197	1140	226	1364	321	1292	334
	2d 100	695	137	925	85	1097	182	1265	144	1201	164
D.	1–5 figures.	439	36	469	46	511	36	539	37	516	29
	Difference.										
	1st 100	357	159	537	151	629	190	825	284	776	305
	2d 100	256	101	456	39	586	146	726	107	685	135
	1–5 disp. imp.										
	1st 100	747	170	977	226	1047	259	1244	240	1021	233
	2d 100	698	97	1056	172	1226	227	1218	272	1148	180
H.	1–5 figures.	441	27	425	22	413	44	384	45	462	46
	Difference.										
	1st 100	306	143	552	204	634	215	860	195	559	187
	2d 100	257	70	631	150	813	183	834	227	686	134
	1–5 disp. imp.										
	1st 100	828	175	1239	179	1432	262	1512	228	1257	343
	2d 100	665	104	1111	255	1417	223	1141	257	721	153
Mca.	1–5 figures.	451	30	510	38	575	63	571	53	509	23
	Difference.										
	1st 100	377	145	729	141	857	199	941	175	748	320
	2d 100	214	74	601	217	842	160	570	204	212	130
	1–5 disp. imp.										
	1st 100	822	225	992	152	1149	210	1303	318	1461	399
	2d 100	636	103	868	149	944	184	1153	300	932	128
Rog.	1–5 objects.	412	31	436	28	443	46	443	29	440	28
	Difference.										
	1st 100	410	194	556	124	706	164	860	289	1021	371
	2d 100	224	72	432	121	501	138	710	271	492	100
	1–5 disp. imp.										
	1st 100	698	195	921	139	1029	206	1041	133	968	209
	2d 100	773	141	920	142	932	136	938	124	872	132
R.	1–5 figures.	537	30	669	42	687	57	775	53	682	71
	Difference.										
	1st 100	161	165	252	97	342	149	266	80	286	138
	2d 100	236	111	251	100	245	79	163	71	190	61
Average of difference											
	1st 100	322	161	525	143	634	183	750	205	678	264
	2d 100	237	86	474	125	597	141	601	176	453	112

This experiment of reacting to the Arabic figures was conducted in the same way as that recorded in Table IX., except

that here the averages are of twenty instead of ten as there. This experiment immediately followed the one just described. The numbers for subject R. are taken from the similar previous experiment of this subject, the averages of which are given in Table IX., as it was impossible to have him for the longer and later experiment. The fourth line of numbers under each subject in Table XI. gives the difference between the time for reacting to the figures and to the first twenty of the disparate stimuli for the same finger. The fifth line gives this difference for the second twenty. At the bottom of the table is given the average difference for all the subjects for the first and also the second twenty reactions for each finger. These were kept separate throughout the table in order to show the effect of practice on the second as compared with the first hundred reactions.

From the average differences for the first hundred we see that it took 322 thousandths of a second longer to perceive one of the five disparate impressions than it did to perceive the figure 1. For the double impressions this time was 525 thousandths more than for the figure 2; for the triple, 634 more than for the figure 3; for the quadruple, 750 more than for the figure 4. and for the quintuple, 678 more than for the figure 5. We thus have a very marked increase in the time needed for perception as the number of disparate impressions increases, with the exception of the quintuple series, where the time is greater than that for the triple but less than that for the quadruple series. This decrease is sufficiently explained by the introspective experiences already given, which showed that this number was frequently reacted to as a total. The lack of a settled method in reacting to this explains why the mean variation is here the largest.

The average differences for the second hundred show that practice produced a decrease in the time required for perception. The mean variations are also in closer accord with the averages, which shows a greater regularity of method. This decrease is shared by all the series, but more pronounced with the quadruple and quintuple than with the others. A record kept of the reactions as to a total in the latter and of subtractions

and reactions as to a total in the former shows these to have been much more frequent in the second hundred than in the first. The effect of practice upon the mental process in first receiving the stimuli was to decrease the tendency to dwell upon their qualities and to cause a diminution of their discreteness. This was accompanied by a decreased prominence of counting in determining the number. This first appeared with the quintuple impressions where a less likelihood of error and a greater familiarity was felt, but later became general. The number of

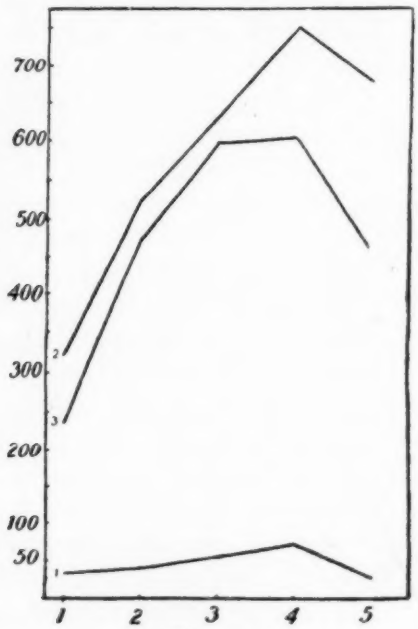


FIG. 3.

stimuli thus came to be associated directly with the right finger for reaction without the image of the name of the numeral coming in between. One subject, *H.*, became finally convinced that it was this rather than a reaction to a total which really occurred for the quintuple impression. Yet for this subject the counting came to be more distinct for this series during the second hundred, and it is of interest to note that this is the only subject for whom practice did not generally decrease the time of this series in the second hundred as compared with the first.

Even here, however, the time for the quintuple is less than that for the quadruple series.

In Fig. 3, the curve marked *1* gives the result of the experiment with similar (visual) impressions in which the tachistoscope was used. The curve marked *2* is made from the first hundred of the disparate impressions, and the one marked *3* from the second hundred. The figures on the left mark gradations in thousandths of seconds, and those at the bottom the number of stimuli given at once. Curve No. 1 (based on Table IX.) begins with 33 σ, rises gradually to 70 σ for the quadruple impression, and sinks to 28 σ for the quintuple. Curve No. 2 (Table XI.) begins at 322 σ, rises abruptly to 750 σ for the quadruple impression, and sinks with the 'total' reactions to 678 σ for

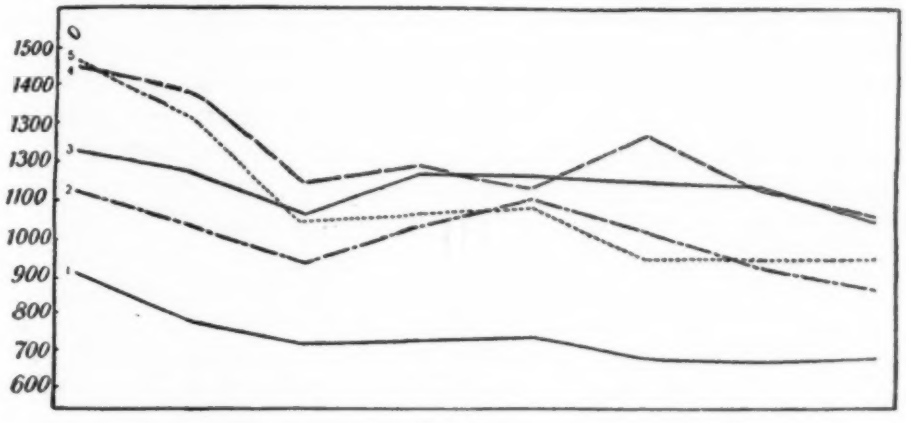


FIG. 4.

the quintuple. Curve No. 3 begins at 237 σ, rises abruptly to 597 σ for the triple impression, then very slowly with the increased method of subtraction for the quadruple to 601 σ, and sinks with the 'total' reactions to 453 σ. The difference of time for the curves at starting, and also the rate of increase of the similar as compared with the disparate stimuli as the number of impressions increased are here shown in a striking way.

Fig. 4 shows the effect of practice on the different series of the disparate impressions. The numbers at the left indicate a gradation in thousandths of seconds. Each curve indicates a

series and is marked with the number of simultaneous stimuli which characterized it. Each point in the curves was determined by averaging groups of twenty-five reaction times, divided equally between the five subjects. The first point in each curve thus came from the first twenty-five, the second point from the second twenty-five in its series, and so on. A variety of ways is used in drawing the curves in the use of dotted and broken lines to make them the more easily distinguished. Until the third point, or the fifteenth reaction for each subject, the effect of practice is very marked in all the series. This is followed by an increase of time most evident in the double and triple series, and then by a general decrease to the end. The effect of practice is most marked with the quadruple and quintuple series. Both curves fall precipitously to the fifteenth reaction, the fifth curve crossing both the fourth and third, and later crossing and recrossing the second. The fourth follows the third in a similar way. If the fourth and fifth series could have been reacted to from the time practice first began, the curves which represent them would probably have begun in the neighborhood of two or three thousand sigma. By subtracting the average which determines the last point of each curve from that for the first point, we get the net decrease of time which practice caused in each series. For the first curve this amounts to 226σ; for the second, 259σ; for the third, 184σ; for the fourth, 402σ; and for the fifth, 521σ. This result is in accord with the effect of practice observed with the counting of disparate impressions in an earlier experiment.

The argument against the distribution of attention, as supported by the reaction times to different numbers of similar objects, is evidently made stronger here, where with dissimilar stimuli the time of perception increased much more rapidly in proportion to the number used. This result is in accord with the fact that simultaneous disparate stimuli cause a displacement when it is attempted to perceive them together. They do not seem to coincide as they actually do in being given, but one is displaced and seems to be advanced or retarded. This result has been common to so many students of this phenomenon, Von Tchisch, Pflaum, Gonnessiat, Exner, Wundt, etc., that it is

beyond question. Exner found this displacement to amount to 44 σ in the case of similar visual impressions. In order to make tactual-visual stimuli appear to come together they had to be separated, when given in this order, by an interval of 53 σ, but when the visual came first, by an interval of 71 σ. Auditory and visual stimuli were displaced 60 σ when given in this order, and 160 σ when given in the inverse order.[1] It was no doubt the presence of this factor as it occurs with similar impressions which prevented the succession from being discerned in the experiment with the successive exposure of letters; and if from the results of the experiments with disparate stimuli it were possible to subtract the time of delay caused by not knowing the stimuli to which reaction would be required, there would doubtless be a reasonable degree of conformity between Exner's figures and my own here also. This displacement would naturally cause an increase in the time needed for perceiving the number of the stimuli, and thus would be a factor in causing an immediate hindrance to simultaneous perception.

A possible argument for distribution, or at least for an overlapping of the processes, for perceiving both the similar and disparate impressions may be based upon the fact that it did not take twice as long to perceive the double impressions as it did the single ones, or three times as long for the triple as the single, and so on. It might be claimed that, providing distribution is not present, each step in the increase of the complexity of the impression should be accompanied by a constant increment of time equal to that for perceiving the single impression. It should be noted, however, that all of these averages are greatly influenced by practice which caused an abbreviation of the process. We should expect the above requirement to be more nearly fulfilled by the first reactions in these experiments, and this is really the case. But even here, it should be remembered that both the experiment with disparate impressions and also those that with similar ones were preceded by a long practice series, during which the time was greatly reduced. There is, then, no need of an explanation of the overlapping, aside from

[1] Exner, S. 'Experimentellen untersuchungen der einfachsten psychischen Processe.' *Pflüger's Archiv*, XI. (1873), S. 403.

the effect of practice. The influence of practice, on the other hand, is the most readily explained by the increased rapidity of action common to all nervous processes as a result of use. The mental acts employed in perceiving these impressions are unquestionably conditioned by the functioning of nervous processes.

11. The Mental After-Image.

The fact of succession in acts of attention in the reaction experiments just described explains why a smaller number of complex than simple objects can be retained from a tachistoscopic exposure, and why the longer time for associating unlike impressions makes it more difficult to perceive these together than for similar ones. The rapid fading out of the mental after-image prevents the retaining of those objects which cannot be immediately rooted by means of the establishing of perceptive relations through apparently exclusive acts of attention. The duration of this after-image would, then, be a condition which determines the number of objects retained. What determines this duration? Its length varies for different people. In the experiment previously described, in which ten simple objects were exposed to find how many could be retained, the duration of the mental after-image was approximately got for each of the subjects. An ordinary stop-watch was used for this from which the time was taken from the exposure until the giving of the number of objects seen by the subject. The gradual fading of the image made it difficult for the subject to indicate the point at which no more objects could be recovered; but the number was given when all distinct ones had been enumerated, and no other seemed obtainable. Also the time required for the experimenter to stop the watch made the time from one to two fifths of a second too long. Table XII. gives the time in seconds

Table XII.

Subject.	D.	H.	Mea.	Mer.	R.	Rog.	All.
Av.	3.1	2.0	2.7	4.7	2.3	1.9	2.8
M. V.	0.3	0.3	0.6	0.5	0.6	0.2	0.4

as read from the watch. The averages under each subject are each from twenty determinations. If we compare this table

with Table VII., in which is given the number of objects each of these subjects saw in a hundred exposures, we see that Subject *Rog.* saw the smallest number and also had the shortest after-image. *Mea.* also ranks third in both tables. But aside from these two instances the orders are not the same. Thus *D.* saw the greatest number, but comes second in the length of his image; *H.* comes next in the number seen, but fifth in length of image. This indicates the presence of some factor, other than the length of the image, which determines the number of objects perceived, probably the individual rapidity of perception. Evidently it was the effect of practice upon this rapidity which brought the perception of the larger number of disparate impressions within the duration of the after-image in the last experiment.

This after-image is apparently the direct product of stimulation in the same way that the ocular after-image is. Either can be disregarded if the attention is otherwise employed, both remaining a certain period to represent the stimulus that has passed. The duration of the ocular after-image depends upon the length and intensity of the stimulation. May not the mental after-image similarly be a product of these? May not the number of objects of the same degree of simplicity seen in a tachistoscopic exposure depend, for a given individual, upon these conditions of nervous excitation?

To test this, a somewhat elaborate study was made by means of the tachistoscope, in which the distinctness and duration of the exposures were varied. The time of the exposure was changed by changing the width of the opening in the shutter. There were three openings, 6.3, 3.5 and 1.1 cm. wide, respectively, and making the corresponding times for the whole exposure 42σ, 32σ and 24σ, or for each letter 23.8σ, 15.0σ and 6.6σ. There were three sets of cards, twenty in each, with the same black letters before used, eight distributed irregularly upon each card. Set *A* was of white cardboard; set *B* was 233.5/360, or about two thirds white, as tested by a color wheel; and set *C* was 77/360, or about one fifth white. Each set was employed in each of the three periods of exposure, thus making nine series for each subject; and as each of the twenty

cards was exposed five times in each series, this made one hundred exposures to a series. There were four subjects who took part in the experiment, thus employing in all thirty-six hundred exposures. The technique of the experiment was conducted in all respects like that with the consecutive exposure of letters. It was found advisable, however, to work equally upon all of the nine series within each hour in order to equalize any influence which might arise from practice or the varying brightness of the daylight from day to day, since electric illumination was not used.

Each card was shown five times with each period of exposure, or fifteen times in all to each subject; but as each subject served but two hours per week in the experiment, and but twenty-seven exposures were made in an hour, the cards could not be expected to be remembered to a great extent. Subject *A* remembered two cards at the end of the experiment as having been seen before, and subject *Y* three. Identifying them as having been shown before did not, however, seem to influence the recalling of the letters. Yet there no doubt was a subconscious influence exerted to some extent; but owing to the method of the experiment this was distributed equally among the different series.

The subjective experiences were in the main similar to those of the experiment with the consecutive exposure of letters, but with an increased tendency to see more letters that could not be recalled. Two subjects found the longest exposure of the white cards somewhat dazzling, and one of these, subject *A.*, saw a few more letters on the medium gray for the same time of exposure in consequence. The size of the exposed surface seemed distinctly smaller with the darkest cards and the shortest exposure, and its outline was less distinctly defined. More letters seemed to be present in the brighter exposures, and also more that could not be given. Table XIII. gives the results of the experiment. As before, the numbers give the letters seen in a hundred exposures. The averages show a gradual and almost constant decrease in the number of letters correctly seen from the white cards with the full opening to the darkest cards with the smallest opening. In the case of this last, the per cent. of

TABLE XIII.

	Subject.	Full Opening.			Medium Opening.			Small Opening.		
		A.	B.	C.	A.	B.	C.	A.	B.	C.
Number seen correctly.	A.	299	305	279	287	274	236	256	195	79
	R.	238	239	234	230	217	211	199	174	85
	S.	228	203	206	209	195	202	190	151	92
	Y.	287	277	267	266	251	246	246	182	111
	Average.	263	256	246	248	234	224	223	176	92
Number seen wrongly.	A.	42	36	42	40	31	40	20	18	31
	R.	69	67	71	70	61	74	81	66	58
	S.	61	73	79	53	53	60	44	37	43
	Y.	71	69	85	83	69	73	65	56	46
	Average.	61	61	69	62	54	62	53	44	45
Per cent. of those wrongly to those rightly seen.	Per cent.	23	24	28	25	23	28	24	25	49
Number misplaced.	A.	3	4	7	8	8	7	1	1	11
	R.	14	12	16	11	11	14	12	11	6
	S.	14	22	18	11	20	12	16	10	9
	Y.	19	18	23	13	16	15	22	4	9
	Average.	13	14	16	11	14	12	13	7	9

those wrongly to those rightly seen has a very striking increase, while the number of those simply misplaced in proportion to the whole number seen has also increased. Evidently this shows that the conditions under which this series took place greatly facilitated the combining of the component parts of letters wrongly. Yet in this series the concentration of the attention was on the whole greater than for any other series, owing to the feeling that more effort was needed to see the letters.

The following curves (Fig. 5) show where and to what extent the number of letters seen corresponded to the time of exposure and the brightness of the cards. The curves are headed at the top in the same way as Table XIII. The dotted line indicates the time of exposure of a single letter for the different series and is graduated to thousandths of seconds on the left. The broken line represents the brightness of the cards and is graduated in corresponding degrees of white as tested upon the color wheel. The solid line represents the number of letters seen in each series and is graduated according to the number of letters seen.

The beginning of the last curve at the left under *A* shows the optimum conditions to have existed where the longest exposure occurred and the brightest cards were used. Under *B*

the curve has sunk in response to the decreased brightness of the cards and the consequently diminished distinctness of the letters. It continues to sink under *C* because of the still darker cards used. Under the second *A* the curve rises slightly to the maximum brightness of the cards, although the time of exposure has been decreased. Under the second *B* the curve falls again on account of the darker cards and this fall is increased for the second *C* in response to the darkest cards, although the

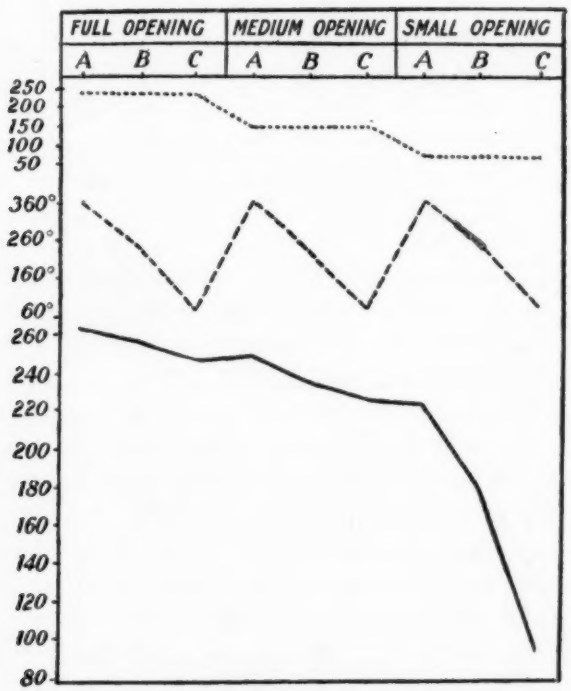


FIG. 5.

exposure has remained constant. The downward progress is again arrested under the third *A*, although not quite so markedly as for the second *A*, from the use again of the brightest cards. But from here on the fall is precipitous, due again to the decreased brightness of the cards.

This shows that while the curve responds to the time of exposure as evidenced by the general downward tendency, it yet responds even more distinctly to the differences of distinctness

in the letters and this response is by far the greatest when the exposure is shortest. The curve hence appears as a function of these two conditions. It resembles in shape that illustrating Weber's Law, and like that law, shows that the effect, here the length of the image, has a large value in proportion to increments of slight stimuli and slight in proportion to those of great stimuli.

Thus judging the length of the mental after-image to be indicated by the number of exclusive acts of attention possible before it fades, we have a close analogy obtaining between the mental and ocular after-images in respect to their duration as resulting from the degree of stimulation. The experiment as a whole, and especially the small number of letters seen with the dark cards and shortest exposure, although there was an unusual effort of attention, plainly supports the conclusion already reached that the number of objects seen from a tachistoscopic exposure depends upon the duration of the after-image rather than upon any special effort to distribute the attention apart from the incidental rapidity of its consecutive acts.

It seems likely, also, that this after-image is an important factor in determining what Wundt regards as the broader limits of consciousness as tested by the power to compare two groups, one coming after the other, of consecutive metronome strokes. He gives the interval between the strokes which favors the getting of the largest number as 0.2 to 0.3 seconds; and since sixteen single strokes mark the limit this would require the retention of the first stroke of a series from 3.2 to 4.8 seconds — a period like that which we found to be equal to the duration of the after-image. Wundt also states that if the strokes are separated by more than four seconds, it is impossible to combine them in consciousness. This is no doubt explained by the fact that the after-image hardly survives this period. It is also possible that the different results obtained in this experiment by different observers are explained by the different lengths of after-image possessed by different people. Thus Bechterew gives 12, 14 and 18 for the number of single strokes that were got from one subject at three different trials in ascertaining the limits of consciousness, while Tschisch found 11 for one subject

and 13 for another.[1] The differences for different subjects which we found in the lengths of after-images (1.9–4.7 sec.) would explain much greater differences of results.

12. Summary.

In the counting of simultaneous series of similar impressions the rate decreased as the number of series increased. The exceptions to this in the double and triple series of visual impressions are readily explained as the result of rhythm. In the counting of simultaneous series of disparate impressions the loss in rate was still more pronounced. These results show that in these simultaneous series with motor paths converging to the same organ of expression distribution is excluded. Experiments with divergent motor paths, which are most successful with disparate processes, require sufficient automatism to make an explanation of the results based upon distribution unnecessary.

Reactions in which the concentration and also attempted distribution of attention were compared showed, by the increase of time and mean variation in the reactions in which distribution was tried, that the attention was not distributed, but fluctuated between the possible stimuli expected.

These results led to the questioning of the interpretation of tachistoscopic experiments as given by Wundt and others. An experiment of this kind, in which the exposure was short, and the objects were presented in succession, showed that although the subjects' experiences were typical of such experiments, the impression was not made conscious until after the exposure was over: so that distribution, if it took place, was not, as has been claimed, coincident with the exposure. Since distribution is not possible during this subconscious period of inertia, the question came as to its presence during the conscious period which followed. To answer this, the experiment was conducted in which the subject reacted differently to different numbers of simple similar objects from one to five. By subtracting from the time thus obtained the time required for reacting in a similar way to figures 1 to 5, the time necessary for perceiving the ob-

[1] See review of article in *Neurologisches Centralblatt*, VIII. (1889), S. 272.

jects over that for perceiving the figures was obtained. This time increased in proportion to the number of objects except in the case of five, which is readily explained by special devices for its quick recognition. Consecutive acts of attention rather than distribution were thus shown to characterize the perceptive process. Since it takes longer to perceive complex objects than it does simple ones, this successive process explains why fewer complex than simple objects can be got from a tachistoscopic exposure. These results were confirmed and made more apparent by the reactions to simultaneous disparate impressions where the time of perceiving the number was greatly increased.

According to introspective experience the limit to the number of objects which could thus be got was fixed by the duration of the mental after-image. The experiment in which the duration and distinctness of the visual impression were varied showed that the length of the image was distinctly controlled by these features of the impression. So that the number of objects got from an exposure depends upon physiological conditions rather than upon a specialized form of mental activity.

In the same way that in learning to read, originally separate objects come to have close perceptive relations, so practice tends to unite into a closer perceptive unity impressions at first combined with difficulty. This was shown in the counting of disparate impressions, in reacting to different screen openings between which it was tried to distribute the attention, and in the reactions to different numbers of similar and also of disparate impressions. As devices for obtaining this unity may be mentioned the rhythm in the counting experiments, where each impression thus came to have a special relation to other impressions, and the grouping which accomplished the same end with simple objects in the tachistoscopic experiments. In the latter experiment some subjects noticed a decreased tendency to group as the experiment progressed, which suggests that an acquaintance with the cards was a substitute for this. This close perceptive or associative relation, which results from practice in combining discrete impressions, accounts for the reënforcement of closely related impressions, as shown by the large number of letters and figures that can be seen, from one exposure, when

they enter into intelligible relations to form words and numbers as compared with arrangements in which this is not possible.

13. Conclusion.

These results seem to support the following conclusions:

Those things which we perceive as single objects are composed psychologically of a group of elements which in many cases were primarily discrete objects of attention, *e. g.*, the color, elements of form, size, and location involved in a letter of the alphabet may each be separate objects of attention, and were obviously so until united into an apparent unity by practice. Those elements which are habitually found together in objects of perception become so closely associated mentally that we are not conscious in recognizing them of the steps which bring them together, because of the decrease of association time. Evidence of the steps which still pertain to apperceptive groups of this kind can be had only by the most exact time measurements, as shown by experiments with words, numbers and groups of similar objects. The increased association time for unlike impressions makes the steps by which they are united more apparent. The conscious difference, therefore, between one object and more than one is a difference in the closeness of association, and is hence one of degree rather than one of kind. Distribution, then, can take place only when the association time has so decreased that succession has disappeared. But at this point the conscious plurality has become a conscious unity, the elements of which are not perceived as such. Hence distribution also does not occur here. Simultaneous distribution is thus seen to be a psychological impossibility.

The duration of the mental after-image easily explains the phenomena that have been ascribed to distribution in tachistoscopic experiments. This, with the subconscious and conscious relating of the elements of a single object, and also of separate objects into quickly perceived complexes, serves to do for perception what the coördination of many nervous discharges does for the production of effective muscular movements. Practice comes to be the great factor in the organic

development of mind, since it brings a constantly increasing number of elements within the grasp of the after-image for immediate perception through the shortening and dropping out of the conscious links which are at first necessary to unite them. We know in general that when two portions of the cortex are stimulated quite or nearly simultaneously, they form mutually reënforcing relations so that afterwards when one is stimulated, the other is excited also. This is illustrated in associative memory where one idea calls up another when it has occurred immediately before or after it. The duration of the mental after-image seems to mark the period of cortical excitation, during which a new excitation may enter into functional relations with a former one, but after the subsidence of which, excitations no longer are able to produce this result. We may thus undoubtedly experience simultaneously excited ideational centers in the cortex, but we have seen that this does not bring about simultaneous consciousness of the corresponding ideas. We may, perhaps, picture consciousness as traveling along the association fibers from one center to another, the path adopted being determined by the degree of intensity of nervous excitation. The distance and degree of resistance between some of these, as in the case of unusual relations of different sensory areas, causes the transition to be sufficiently slow to be clearly noticed; but in other cases, where the centers are close together, or where frequent use has decreased the resistance, the transitive stage is of much less duration, and may become so short as to make the impressions appear to have completely fused, introspectively, and to have reduced the actual time to five or ten sigma.

SOME POINTS OF DIFFERENCE CONCERNING THE THEORY OF MUSIC.

BY PROFESSOR MAX MEYER,

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Mr. Dixon's excellent criticism in *Mind*[1] of my theory of music[2] suggested to me to write the following pages, which, I hope, will make some details of my theory clearer than I was able to make them in the publication which he reviewed. I shall further have to refer to an article of Lipps[3] and an earlier one of my own[4] and therefore mention them here at once.

Mr. Dixon states that according to my experience melodic relationship does not exist between two tones the ratio of whose vibrations involves primes higher than 7. The latter part of this statement does not quite agree with what I wished to say. One must not infer that there is relationship whenever the primes involved are lower than 11; I do not believe to experience, *e. g.*, relationship with 7–9, or 7–15. I am extremely glad, however, for his use of the word experience. My theory, indeed, is not intended to be a mere dialectic, but the systematized expression of my experience. I regret as much as the reviewer that I have not tried my experiments on a large number of unbiased observers. This is not caused by any belief in my own self-sufficiency, but by circumstances over which I have no control. To find unbiased observers for experiments of this kind is much more difficult than it seems to be. Further, the methodical difficulties of such experiments are extraordinarily great; and to make the experiments in such a way as they ought to be made requires an amount of time of which those

[1] *Mind*, New Series, 44, Oct., 1902, pp. 567–571.

[2] Max Meyer, 'Contributions to a Psychological Theory of Music,' *University of Missouri Studies*, I., 1, 1901, pp. 1–80.

[3] Th. Lipps, 'Zur Theorie der Melodie,' *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, Bd. 27, 1901, pp. 225–263.

[4] Max Meyer, 'Elements of a Psychological Theory of Melody,' PSYCHOLOGICAL REVIEW, VII. (3), 1900, pp. 241–273.

have no conception who never attempted it. The chief cause why I did not furnish my theory with a greater material of data is the well-known fact that a college professor has plenty of other duties besides making experiments. Therefore the experience referred to is mostly my own experience; but this does not mean that it is only a casual observation. I have become convinced of the value of my theory, and I regard this a sufficient reason for publishing it. Nevertheless I may be wrong in many respects, and I shall be thankful for any objections brought forward in so truly a scientific spirit as the reviewer's.

The melodic relationships which I believe to experience are expressed by the symbols: 2–2, 2–3, 2–5, 3–5, 3–7, 2–7, 2–9, 2–15, 5–9 and 5–7. No account is taken of 2 as a factor. This is of the utmost importance, as it simplifies the theory immensely. It is interesting to me to see that a mathematically trained mind like Mr. Dixon's does not object to this simplification, whereas another reviewer called this omission of all factors which are pure powers of 2 unwarranted, without being aware, obviously, of the fact that musicians for centuries have done what amounts practically to the same in naming all tones by the same letter which differ by multiples of an octave. This fact, with which he was familiar since his earliest youth, seemed to him quite natural; but my omission of the powers of 2 he could not grasp and therefore rejected it.

It seems to me that I have been greatly misunderstood with respect to my psychological definition of a 'tonic.' This tonic is not identical with what musicians call a 'key-note.' The word key-note, as I would like to use it, refers only to the system of musical notation which we desire to employ in order to write down a certain tune; to the sharps and flats which we use. It has no psychological significance — *i. e.*, in my writing. I expected to make this clear by not usually employing the common musical notation. If we use the common musical notation, if we distinguish a tone by calling it key-note, we should of course select as a key-note a tone which is psychologically emphasized in the melody above the others. But since this emphasis may be based on very different psychological facts I shall not here introduce 'key-note' as a scientific term. A scientific

term must have a distinct meaning, and not more than one meaning. My definition of a 'tonic' is correctly quoted by Mr. Dixon: It is a pure power of 2 when combined with melodically related tones, *i. e.*, with 3, 5, 7, 9 or 15. Such a combination of successive tones we wish to have end on no other tone but the power of 2. I do not mean that, physically, one *cannot* close on one of the other tones; of course one may, but he produces then an æsthetic effect that is hardly ever desired. Further, I do not mean that, whenever we are satisfied by a certain tune ending on a certain note, this note must be a tonic as above defined. It may be a tonic only relative to some of the tones contained in the whole melody; not relative to others, not to the melody as a whole. Indeed, it may not be a tonic even relative to a single other tone, and yet we may be satisfied by the ending, perhaps for no other reason but that we know that this tune ends on this tone, and if it did not, it would not be the tune we expect to hear. This multiplicity of the causes of a certain æsthetic experience is the greatest of all the obstacles to experimental research in this field. This will do away with Mr. Dixon's objection to my theory that it necessitates the use in a complex piece of music of very large numbers to represent the tones. "If, for example, a melody, in ordinary language, modulates to the subdominant key, Professor Meyer would say: 21 becomes a partial tonic. The subdominant of the new key would then be represented by 441, which contains the square of 7, and it is not given in Professor Meyer's scale." Herein I cannot agree with the reviewer. I omitted the square of 7 in the complete scale because, after a good deal of experimental work, I became convinced, and am convinced, that the square of 7 is never used in actual music. I should *not* say that 21 becomes a partial tonic, since I should not represent this subdominant by 21, nor the key-note in this case by 2. On the contrary, I should say that this piece of music, as a whole, has no tonic. What this means I hope to make clearer by the following.

I define the complete musical scale as the infinite series of all products of the powers of 2, 3, 5 and 7; because within this scale is to be found any melody imaginable. Of course, we cannot write down a complete infinite series. Let us here write

down the series up to 405. The reader will see that for the present purpose this is sufficient. Let us also omit all those numbers which are derived from smaller ones by multiplication with a power of 2. We do not need them here. We then have the following series:

2, 3, 5, 7, 9, 15, 21, 25, 27, 35, 45, 49, 63, 75, 81, 105, 125, 135, 175, 189, 225, 243, 315, 375, 405.

It seems to be a simple fact that the æsthetic effect of a melody depends on both these conditions: variety and closeness of relationship. (I do not mention here the tonic effect, since this may be absent.) But Lipps, Dixon and others add a further condition for a succession of tones to be a melody. Mr. Dixon says: "To apprehend a succession of tones as a melody is, psychologically, to apprehend relationships which imply the relation of each note to a tonic, just as on the physical side to represent the notes by numbers having simple numerical ratios is to imply a number which is the greatest common measure of all." I must say, on empirical grounds, that this seems to me an *a priori* assumption. I do not find it necessary that in a melody all tones must be related to a certain single tone, but merely that *each tone must be related to some other tone, and any group of tones to some other group, temporally near enough, so that the relationship can take effect.* Neither can I admit that the greatest common measure has anything to do with musical theory; else, why should there be relationship between the tones 15 and 16 and no relationship between the tones 10 and 11? To speak of the greatest common measure means to go beyond the facts actually observed. This is one of the reasons why I used as symbol for the power of 2 the figure 2 and not 1; I anticipated that the figure 1 would falsely suggest something like the greatest common measure.

The conditions of melodic effect, variety and closeness of relationship are to some extent opposed to each other. If we use for a melody six different tones in intervals of octaves, there is the greatest closeness of relationship, but no variety; there is only a single relationship in the melody, namely, 2–2.

If we play the tones successively as we find them in the complete scale, there is enough variety of relationship, but hardly any closeness to speak of. *Actual music, therefore, will be a compromise of these two conditions.* This consideration can help us to derive from the complete scale smaller scales, which are more easily applicable to special pieces of music.

Let us determine the greatest number of tones which are all mutually related. Here, however, we may either make use or not of the particular effect of a power of 2 when combined with a related tone, the tonic effect, as I have called it. Let us first use this effect in the strongest possible manner, and then try the other way of avoiding this effect as far as this is possible.

Which, then, is the greatest number of tones, including 2, which are all mutually related? I must suppose, of course, that the reader will always keep in mind those relationships which I experimentally determined: 2–2, 2–3, 2–5, 3–5; 2–7, 3–7, 2–9; 2–15, 5–7, 5–9. Now, if we look over the complete scale as we find it above, we have to accept 2, 3 and 5. But we cannot accept all of the next three tones, 7, 9 and 15, because 7 is not related to either 9 or 15. We therefore have to choose between 7 on the one side, 9 and 15 on the other. Since the addition of two tones makes the number of tones greater than the addition of one, the above condition compels us to accept 9 and 15 and to reject 7. We then have the five tones 2, 3, 5, 9, 15, which are all mutually related. No further tone of the complete scale is related to all of these five.

We agreed to make the tonic effect as strong in these scales as possible. The best method of introducing further tones is then, obviously, to use as a secondary tonic the tone which is most closely related to the tonic. Of the four tones 3, 5, 9 and 15 the tone 3 has by far the closest relationship to 2. If we make 3 a secondary tonic and use relationships of the first degree only, 9 and 15 offer themselves. But they are already in the scale and need not be added. If we use relationships of the second degree we have to add 21 and 27. Our scale is then the following:

2, 3, 5, 9, 15, 21, 27.

If we use also relationships of the third degree, we have to add 45. Our scale is then this:

2, 3, 5, 9, 15, 21, 27, 45.

He who desires may add further tones by making another of the original tones a secondary tonic. If he does so, he will become able to have within the whole melody a greater number and a greater variety of partial melodies, each containing a secondary tonic. According to my experience, however, music with a primary tonic is hardly ever very complicated, so that the addition of further tones to the scale is not of much practical significance. The table below permits us to compare the different scales. That 2 has been given the musical name F is quite arbitrary.

SCALES WITH A PRIMARY TONIC.

F	F#	G	G#	A	A#	B	C	C#	D	D#	E	F
2	—	9	—	5	—	—	3	—	—	—	15	2
2	—	9	—	5	21	—	3	—	27	—	15	2
2	—	9	—	5	21	45	3	—	27	—	15	2

and so on.

Our second intention was to determine the greatest number of tones mutually related, while neglecting as much as possible the peculiar effect of a tonic. We therefore omit 2 entirely and accept three and five. 7 has to be omitted again, because its acceptance would necessitate the omission of the *two* tones 9 and 15, with which 7 is not related. We have further to add 45, this being related to 3, 5, 9 and 15. No further tone of the complete scale is related to all of these five:

3, 5, 9, 15, 45.

If we desire to introduce further tones and yet have the closest relationship possible, we must find out to how many of the above five tones each further tone is related and then select the one or those which show the greatest number of relationships. Below we see the tones of the complete scale which are left, each one marked with as many stars as it shows relationships. *E. g.*, 7 is related to 3 and 5, but not to 9, 15, 45; therefore it has received two stars. And so on.

7	21	25	27	35	49	63	75	81	105	125	135	175	189	225	243	315	375	405
*	*	*	*	*	–	*	*	*	*	–	*	–	–	*	–	*	–	*
*	*	*	*	*		*	*	*			*							
	*	*	*				*				*							
			*															

Only 27 has four stars; all the rest have three or less. We therefore add 27 to our scale, which now is the following:

3, 5, 9, 15, 27, 45.

The remaining tones of the complete scale are again marked with as many stars as they show relationships with the tones of this last scale.

7	21	25	35	49	63	75	81	105	125	135	175	189	225	243	315	375	405
*	*	*	*	–	*	*	*	*	–	*	–	*	*	*	*	–	*
*	*	*	*		*	*	*			*							*
	*	*			*	*	*			*							
										*							

135 has in this case four relationships; no other tone an equal number. We therefore add 135 and have the scale:

3, 5, 9, 15, 27, 45, 135.

The remaining tones are the following:

7	21	25	35	49	63	75	81	105	125	175	189	225	243	315	375	405
*	*	*	*	–	*	*	*	*	–	–	*	*	*	*	–	*
*	*	*	*		*	*	*				*			*		*
	*	*			*	*	*									*
						*	*									

75 and 81 have in this case more relationships than the rest. We therefore add 75 and 81:

3, 5, 9, 15, 27, 45, 75, 81, 135.

The remaining tones are:

7	21	25	35	49	63	105	125	175	189	225	243	315	375	405
*	*	*	*	–	*	*	*	*	*	*	*	*	*	*
*	*	*	*		*	*			*	*	*	*		*
	*	*			*				*					*
		*												*

25 and 405 have more relationships than the rest and are added to the above scale:

3, 5, 9, 15, 25, 27, 45, 75, 81, 135, 405.

The remaining tones are:

7	21	35	49	63	105	125	175	189	225	243	315	375
*	*	*	–	*	*	*	*	*	*	*	*	*
*	*	*		*	*	*	*	*	*	*	*	*
	*	*		*				*	*	*		

21, 35, 63, 189, 225 and 243 have three relationships each and are added to the last scale:

3, 5, 9, 15, 21, 25, 27, 35, 45, 63, 75, 81, 135, 189, 225, 243, 405.

If one wants to continue this, he must take into consideration a greater part of the complete scale than we did. We stopped at 405. But a greater extension of these scales is of little practical significance. The table below permits us to compare these scales.

SCALES WITHOUT A PRIMARY TONIC.

G	G#	A	A#	B	C	C#	D	D#	E	F	F#	G
9	—	5	—	45	3	—	—	—	15	—	—	9
9	—	5	—	45	3	—	27	—	15	—	—	9
9	—	5	—	45	3	—	27	—	15	—	135	9
9	75	$\binom{5}{81}$	—	45	3	—	27	—	15	—	135	9
9	75	$\binom{5}{81}$	—	45	3	$\binom{25}{405}$	27	—	15	—	135	9
9	75	$\binom{5}{81}$	21	45	$\binom{189}{3}$	$\binom{25}{405}$	27	225	$\binom{15}{243}$	63	135	$\binom{35}{9}$

and so on.

An important fact, to be learned theoretically from the above scales, is the relative insignificance of 7, although the complete omission of 7 would prevent a truly scientific theory of music. In the scales with a primary tonic the pure 7 is absent; so is it in the scales without a primary tonic. In the latter the products containing the factor 7, *i. e.*, 21, 35, 63, 189, enter into the scales according to closeness of relationship only comparatively late, after many other tones. The square of 7 never enters into any of these scales. This agreement of my theory with the generally recognized fact of the comparative insignificance of the number 7 for musical theory proves the correctness of the principles of my theory, the correctness of my observations concerning the laws of relationship of any two tones.

In the scales with a primary tonic there can be no question as to *which tone should be the key-note; of course the tonic.* But in the scales without a primary tonic this depends or may depend on many different conditions. The tones which are most commonly used as key-notes are 9 and 5. Why these tones are used thus, I shall now try to make clear.

Let us study more closely the relationships contained in the series 3, 5, 9, 15, 27, 45, 75, 81, 135. There is no primary tonic, but several of the tones are secondary tonics. We re-

member that the tonic effect is particularly strong in the relationships 2–3 and 2–5. Let us therefore see which of the tones of the above series can be combined with tones of these two relationships.

3	5	9	15	27	45	75	81	135
9	15	27	45	81	135			
15		45	75	135				

We see that only 3, 9, 15 and 27 can be combined with tones of both the relationships 2–3 and 2–5. Let us for the present discussion call these four groups of three tones each by the names group 3, group 9, group 15 and group 27. We should now determine how these groups are interrelated. Each combination of two groups makes nine relationships possible. The table below shows how many of these nine are lacking.

Group 3 combined with group 15 (–2), with g. 27 (–3).
" 9 " " " 15 (–2),
" 15 " " " 3 (–2), " " 9 (–2), with g. 27 (–3).
" 27 " " " 3 (–3), " " 15 (–3).

The tones of group 3 and group 9 are all mutually related. Group 3 combined with group 15 lacks two relationships, 75 not being related to either 3 or 9. This is expressed by the parenthesis (–2). Group 3 combined with group 27 lacks three relationships, 3 not being related to either 81 or 135, 15 not to 81. This is expressed by the parenthesis (–3). Group 9 combined with group 15 lacks two relationships, 75 not being related to either 9 or 27. Group 15 combined with group 27 lacks three relationships, 81 not being related to either 15 or 75, 27 not to 75. The group that is most deficient in relationships to the other groups is therefore 15; the one that lacks only two of twenty-seven relationships is the group 9. If we have to emphasize a certain tone melodically, to make it the key-note, nothing is more natural than that we choose the tonic of this group 9–27–45 as the key-note. *Therefore I have made 9 the key-note.*

The above explanation is somewhat similar to that of Lipps. The most important difference is that he needs about fifteen pages in order to explain what I explain on a single page.

Lipps insists on calling the tones 3 and 27 in the above series 'dominants.' Any scientific term should be definable; but I have not been able to discover any definition of 'dominant' which could consistently be used. On the other hand, I do not see of what advantage it is to use the word dominant. Nothing becomes clearer or simpler by using the word. If 3 and 27 are to be distinguished by special names, I do not see why 15 should go without; I propose the name of duke or prince.

It is decidedly dangerous to call 3 and 27 in the above case dominants. Musicians invariably call F and G dominants in any music written in the key of C. But we should remember that F and G in other scales, *e. g.*, the scale 2, 9, 5, 21, 3, 27, 15, 2, have psychological characteristics quite different from those we found here.

Nearly all music that is highly complex, particularly vocal music for a choir of several voices, polyphonic music like organ fugues, and whole operas, seem to be represented by scales without a primary tonic with 9 as a key-note. The scale can of course be further extended than we used it above; remaining, however, within the complete musical scale. Within the complete scale there are many possibilities of partial melodies containing a partial tonic. The greater number of arias in operas are partial melodies of this kind.

Here may be mentioned a fact which has been much discussed by the theorists. It is possible to use in a piece of music written in the key of C (9) both these chords; D-G-B (81–27–135) and D-F-A (5–3–15). This does not cause us the slightest theoretical difficulty. We see in our scales that the tone D on the piano is either 5 or 81. There is no reason why in our music we should not use now the one, now the other. But those whose adhere to the 'diatonic scale' of Zarlino-Rameau-Helmholtz do not get off so easily, since their scale contains only 81, not 5. In order to explain so simple a fact, a single chord, they have to introduce such a complicated theory as that of 'modulation.' Indeed Helmholtz says, though somewhat reluctantly, that this chord D-F-A in the key of C is 'eine beginnende Modulation über die Grenzen der C-Durtonart hinaus.' If simplicity is a requirement for a theory to be called

scientific, no further criticism is necessary. I might easily mention a hundred similar cases, if it were not for the waste of paper and printer's ink.

In music without a primary tonic, besides 9, 5 is frequently used as a key-note on which a melody ends. I shall now try to answer the question why this ending on 5 is 'satisfactory.' Dixon says in his review: "There are some respects in which Professor Meyer's paper is disappointing. In particular he throws no light on what to the psychologist is one of the most interesting problems presented by music, namely, the peculiar æsthetic effect of minor melodies." It is disappointing to me to read this, as I actually thought to have thrown some light on this problem. I shall try to do so more successfully now. Let us consider the æsthetic effect of a melody made up of the tones 3, 5 and 15. 3 is a tonic relative to 15. 5 also is a tonic relative to 15. When we hear alternately these three tones, each time when 15 sounds we experience the strong desire to hear now 3; but also the desire to hear now 5. Fortunately, it is possible to hear both at the same time in a chord. But when we hear both 3 and 5, our attention is attracted by two sensations almost equally; indeed, we may say equally. The relationship 15–5 is closer than the relationship 15–3; but not very much. And while this makes the tone 5 psychologically more active than 3, there is another important factor. The relationship between 5 and 3 is very close. I have repeatedly pointed out in other writings that closeness of relationship tends to spread our attention over different tones, prevents our attention from remaining concentrated upon a single tone. (This is very important in the process of analyzing.) If one of the two tones 5 and 3 were a tonic relative to the other, our desire to return to the tonic would render that tone predominant. But this is not the case. Such an experience is something unusual; our usual experiences end in definite reactions. This wavering between two sensations has a strong emotional effect, which may be fittingly described by the German proverb: Wer die Wahl hat, hat die Qual. Our experience is highly 'unsatisfactory.' And yet it is very satisfactory in another way; just as it is satisfactory to see the sufferings of the hero in a drama.

If we regard one of the three tones of the melody as the key-note, it is natural to take either 5 or 3, not 15. We select 5, because this has the closer relationship to 15. We may of course construct a melody of more than the three tones 3, 5, and 15. But there are no other three tones in the complete scale (excluding 2) which are united by so close relationships as 3, 5 and 15. The addition of further tones does not, therefore, alter our conclusions as to *the tone which should be called the key-note, namely 5*.

	C	C#	D	D#	E	F	F#	G	G#	A	A#	B	C	
Tonic	2		9		5	21	45	3		27		15	2	Major
Atonic	9	75	$\binom{5}{81}$		45	3		27		15	63	135	9	Major
	5		45	3	25	27		15	63		9	75	5	Minor

The above table shows how the three types of music which we have to distinguish upon psychological grounds are related to the musical notations of music as music in major and minor keys. The three types are represented by three arbitrarily selected series of tones of which a melody of each type may be constructed. Of course, fewer tones may be used or further tones be added. The table shows that tonic and atonic music are by no means identical with major and minor music. But minor music is never tonic.

In a similar manner as by Dixon, I have been misunderstood by Lipps in his recent article quoted in the beginning of this paper. I regret that Lipps in his discussion of my theory does not sufficiently distinguish between what I mean by 'tonic' and what he means by 'Tonica.' He calls Tonica that tone on which any melody satisfactorily ends. I call tonic that tone which is a pure power of 2 relative to some related tone. Lipps implies that, whenever a melody ends on a certain tone, I call and must call this tone tonic and represent it by a pure power of 2. This idea has never entered my mind. On the contrary, I believe that the greatest error which can be made in musical theory, is to assume that there is only one cause of a satisfactory ending of a melody. That the final tone is a 'tonic' is one cause for a satisfactory ending; but I have never denied that a melody can satisfactorily end from many other causes. In my first publication concerning this matter (PSYCHOLOGICAL REVIEW) I

published the intonation of a melody which ends on 9. To be sure, the psychological effect of such a melody is different from that of a melody containing and ending on a tonic. But there is not a single word in any of my publications, so far as I am aware, stating that a melody which does not contain a primary tonic (2) and, therefore, does not end on it, for this reason has no satisfactory ending, is disagreeable, is ugly. This is one of the prejudices I am fighting, that a certain æsthetic effect must have a single cause, can have no others. Our psychical processes are not so simple. Lipps then proceeds to assert that according to my theory a succession of tones that does not contain a 'tonic' cannot be a melody at all, since 'the most elementary law of my theory' is that a melody must contain a tonic and since each melody naturally has a final tone, which Lipps calls 'Tonica.' The confusion is here caused by Lipps' unwillingness to grant me the right of calling something tonic which differs from what he calls Tonica. Of course, each melody must have a Tonica, *i. e.*, a final tone. But my 'most elementary law of melodic succession' says nothing of the Tonica, the final tone of any arbitrary melody, but merely states ('Contributions,' p. 24): 'that no hearer is satisfied, if *after having heard* once or more often the tonic 2 he does not find 2 finally at the end of the melody.' I did not suspect that any one could understand this 'after having heard' otherwise than as a conditional clause.

On page 253 of his paper Lipps proves: Meyer verkennt das Wesen der Melodie. One of the premises of this conclusion is the following: Immer, wenn C 'Tonica' ist, soll, nach Meyer, F zu C im Verhältnis von 21–2, A zu C im Verhältnis 27–2 stehen. This premise is imaginary. That I do *not* represent every key-note by 2, Lipps might have noticed. *E. g.*, in Chapter VI. (4) he might have found that among the numbers representing the whole of Schubert's Heidenröslein 2 does not appear at all, in spite of the fact that the piece has a key-note, a 'Tonica,' being written on page 55 in the key of C. If Lipps should think that my interpretation of *his* term 'Tonica' is incorrect, I challenge him herewith to give a clear-cut and universally applicable definition of what he means by Tonica.

A very brief, but equally unconvincing refutation of my theory is to be found on page 234 of Lipps' article in the paragraph ending with the words: Meyer's Theorie ist also falsch. Lipps speaks of a melody made up of the tones 2, 3, 5 and 7. In order to understand the argument, it is well to multiply all the numbers with 3, so that we have the tones 3, 9, 15 and 21. The relationships are not altered by this arithmetical procedure. Lipps then rightly states that according to my theory a melody made up of the tones 3, 9, 15, 21 cannot satisfactorily end on any other tone but 3. *But*, he adds, *the melody ends most satisfactorily only on 2.* Now, I do not see how, logically, a melody made up of the tones 3, 9, 15, 21 can end on a tone which is not among those tones of which the melody is made up. To my mind the final tone of a melody is as much a part of that melody as an animal's tail is a part of that animal's body. That a melody made up of the tones 2, 3, 9, 15, 21 can satisfactorily end only on 2, is exactly what I have been preaching all the time, and I am delighted to see this confirmed by Lipps. But I do not see how this proves: Meyer's Theorie ist also falsch. The succession of the tones G-B-d-f-G-c is one which we have heard innumerable times, so that it would be most wonderful if we did not expect, after having heard the separately quite unusual succession G-B-d-f-G, to hear a final c. We should never forget that one effect may have more causes than one.

On page 260 Lipps says, with the apparent intention of contradicting me: Why should not F : C in the key of C be represented by both ratios simultaneously, 21 : 16 *and* 2 : 3? That my answer to this question would not be unconditionally in the negative, Lipps could know if he would look at the complete scale (this scale was published in order to be used thus), where he can find that, for C = 9, F is represented by both 189 and 3; so that in this respect there is perfect agreement between Lipps and me. Only I do not believe that the tone F on the piano can *simultaneously* act as 189 and 3; this seems to me in contradiction to all our general psychological experience, as I have stated already in my Contributions. But it may act now as 3, later as 189, then again as 3, and so on.

Musically trained persons, however, frequently deny that F in the key of C can ever be 21 to 2. Their theory as well as their practice have usually made them so accustomed to the chord F-A-C in the key of C, that they cannot help imagining this chord when they hear F. Save this artificially acquired habit, no experiential facts necessitate the use of the chord F-A-C in each and every music written in the key of C. One can write a most beautiful accompaniment of a song made up of the tones 2, 3, 5, 9, 15, 21, 27, C = 2, without using at all the chord F-A-C. But musical theorists rarely care for experiential facts. The chord F-A-C is 'the chord of the subdominant,' and not to use it would be irreverence shown to the musical Idol Subdominant. If one insists upon using, in the key of C, the chords C-E-G, G-B-D *and also* F-A-C, — and there are of course no physical means to prevent any one from doing it — then indeed C cannot theoretically be 2 or F 21.

In case the chord F-A-C is used, one must select the tones from the series which I have characterized above as atonic and major, the key-note C being 9. Should we use in the music the tones 9, 81, 45, 3, 27, 15, 135, but omit 5, as we may of course use any smaller number of tones than we find in any of the above series, then my theory seems to be confronted with an apparent difficulty. But the difficulty is merely one of formal logic, not a scientific one. Namely all the symbols can be divided by 3, and the resulting series is: 3, 27, 15, 2, 9, 5, 45. I called this series *atonic*, but since it contains 2, it seems to be *tonic*. Logically, this is contradictory; but scientifically, there is no contradiction. The tone 2, which is numerically a tonic, does not act psychologically as a tonic in this case, because the tonic effect is overpowered by another effect. I have shown on an earlier page that of the three groups C-E-G, G-B-D and F-A-C, or in numerical symbols 9-45-27, 27-135-81 and 3-15-9, or after division by 3 the groups 3-15-9, 9-45-27 and 2-5-3, the group C-E-G is by far more closely related to both the other two groups, than G-B-D is to the remaining two, or F-A-C to the remaining two. This closeness of relationship gives to the group C-E-G such a great psychological effect, that the tone C becomes the chief tone, and the tonic effect of F

becomes insignificant. Scientifically, therefore, there is no contradiction. That the tonic effect is actually suppressed is caused by the peculiar combination of relationships in this special case, as above described.

It is a very important fact which the experimenter should never forget, that such habits as the one just mentioned make most musically trained persons perfectly incapable of acting as observers in certain cases of experimental investigation concerning intonation. They are one-sidedly trained and therefore not unbiased.

Lipps says in conclusion: A melody is an oscillation between the key-note, its fourth and its fifth. If he had said, *certain* melodies are such oscillations, I should consent. But very many are decidedly not such oscillations, but structures of quite different types.

Let me close with some general remarks concerning the method of investigation. There has been, for so many centuries, too much deduction from *a priori* principles, and too little induction from specialized experience. The deductive, as well as the inductive method, has its advantages and its disadvantages. The inductive method does not lead at once to completeness, as the deductive does, but leaves a great many questions open. So I am blamed by Lipps and others for not answering this or that question. But if the principle of deduction is wrong, the complete theory is a complete failure, whereas the theory resulting from the inductive method is right so far as it goes. I believe that the only way of finding the æsthetic laws of music, and of melody in particular, is to determine experimentally the æsthetically most effective intonation of a melody and then to analyze the melody, in order to see how the different relationships are combined so that this effect results. But when I read publications concerning musical theory, I almost invariably get the impression that the author believes in only one, more or less narrow, principle of explanation and assumes, without having made any experiment at all, that a more effective intonation is simply impossible. I believe that the basis of all experimental work along this line must be the complete musical scale as developed in my publications; not any small scale of arbitrarily

selected tones. I made a number of experiments — as many as time permitted; I found that some of the melodies contained 2, some did not. As I said in my publication ('Contributions,' p. 20): 'Pitches represented by the number 2 I shall call tonics,' so I expressed my experience by saying: there are some melodies which contain a tonic, some which do not. There is not a single sentence in my publication, in which I identify a 'tonic' with what Lipps calls a 'Tonica,' or what others call a key-note, or what not. If any one assumes this identification, I cannot prevent him from doing so; but I do not wish to be responsible for the consequences. My experimenting taught me that it would be advantageous for my structural analysis of music to distinguish '2' by a special name, a scientific term; and I believed and still believe that the term 'tonic' is most appropriate. That the word is often used in a different sense, is regrettable; but what word is not used in more than one sense? It certainly is not used in more than one sense in my publication. I openly confess that I have little reverence for a terminology which when seen under the light of experiment discloses no other claim for being respected than the sacredness of old age.

DISCUSSION.

AN ILL-CONSIDERED COLOR-THEORY.

That the making of color-theories goes on apace is a most healthy sign of intellectual activity — a sign that there is a wide-spread feeling of the utter inadequacy of the theories of Helmholtz and of Hering. These are both theories which served a useful purpose in their day, as a means of holding together the vastly complicated facts of color-vision, but they are both wholly inadequate to represent our present knowledge of the subject. The theory of Ebbinghaus met certain logical requirements which must be made of any theory in a very satisfactory fashion, but it was unfortunately wholly in discord with facts discovered immediately after it was brought out, and it has now been withdrawn by its author.[1] It is much to be regretted that Professor von Oppolzer has not been any more successful in meeting the conditions of a successful theory.

The theory of color here laid down[2] may be characterized as, in the first place, a 'return to Goethe.' The author considers that Newton did not sufficiently emphasize the subjective character of color — and this in spite of the fact that Newton states in the clearest terms that color as an experienced sensation is something entirely disparate from the cause of color — a given wave-length or combination of wave-lengths — in the external world. The purely metaphysical (or non-scientific) standpoint from which the problem of color is approached in the paper before us may be inferred at once from the disquisition which meets us already on the second page on the subject of total color blindness (or, as it ought always to be called, choosing one word rather than three, achromasy). What the sensations of the achromatic really are, we are told, it is possible to infer, after frequent conversations with them, by means of the fact that color sensations for normal individuals are accompanied by certain feelings, certain

[1] In a very interesting investigation of Fechner's colours which has lately appeared in the *Journal of Psychology*, the results obtained are regarded as confirming the theory of Ebbinghaus, exactly as if that theory were still in existence. But surely an author must be conceded the right to withdraw his own theory when the occasion demands it!

[2] 'Grundzüge einer Farbentheorie,' Prof. Dr. Egon Ritter von Oppolzer, *Ztsch. f. Psychol. u. Physiol. der Sinnesorgane*, XXIX. (3), 183–203.

æsthetic effects, which we discover to be wanting in these defectives. Even after we have made out that the spectrum looks alike to the totally color blind throughout its whole extent, how can we tell that it does not look to them all red or all green or all blue (as the Helmholtz theory would require us to believe)? "Durch die Art, wie er seine Empfindungen beschreibt, was nur durch Angabe von aesthetischen Wirkungen möglich ist, erhalten wir die Gewissheit, dass er alles so sieht, wie wir, wenn wir Kreide, Schnee, Tageslichte ansehen." This is very wonderful! Is our author really so acute that he can tell, when his patient sits in front of, say pink and white ice-cream, whether the æsthetic feelings which he describes are those appertaining to the sensation pink or to the sensation white? If so, his powers of psychological insight are something marvellous, and he ought to be able to revolutionize our whole science, to lay its foundations broad and deep as they have never been laid before, if he will but bend himself to the task. And would the same principle, may we ask, enable us to pick out also the sensations which remain intact in the case of the partially color-blind? We can easily make out from what they tell us that their sensations are of two kinds only, as the entire spectrum moves before their eyes. But what two? Are there four sorts of æsthetic feelings attached to the four colors, so accurately characterizable that we can infer from our conversation with the patient that red and yellow and green all give him the sensation of yellow alone? If the thing can be so easily made out as this, our ancestors were very foolish to believe as they did for several generations (misled by the theory of Young and Helmholtz) that when the cherries and the cherry-tree leaves looked alike to these defectives, they saw them sometimes both green and sometimes both red (and that yellow was a color-sensation of so little importance that it was not necessary to mention whether they saw it at all or not). A simple examination into these æsthetic feelings, according to our author, would have settled this question correctly many years ago — in fact, upon the first detection of color blindness. What a pity that this idea did not occur to Dalton, who concerned himself much about the character of his color sensations! Even William Pole, who showed wonderful acuteness in this subject, overlooked this method. The fact that the question has been wholly set at rest by the occurrence of individuals who are achromatic and dichromatic in one eye only seems not to have attracted the notice of Professor v. Oppolzer.

To return to the contributions of Goethe to color theory — he has shown himself to be, according to our author, a wonderfully fine ob-

server in all questions which concern 'die innere Anschauung,' as appears at once from this passage of his among others: "Die Farbe sei ein elementares Naturphänomen für den Sinn des Auges, das sich, wie die übrigen alle, durch Trennung und Gegensatz, durch Mischung und Vereinigung, durch Erhöhung und Neutralization, durch Mittheilung und Vertheilung und so weiter manifestirt, und unter diesen allgemeinen Naturformeln am besten angeschant und begriffen werden kann." This passage our author himself characterizes as 'allerdings höchst dunkël,' but he takes it as proving 'zur Genüge that Goethe regarded color as a purely psychological phenomenon, 'das auf innerer Gegensätzlichkeit beruht.' No one can have glanced at Newton's writings without seeing that he too regarded color as a psychological phenomenon; to say that it rests upon an 'innere Gegensätzlichkeit' is a statement that would not be contradicted by saying that it rests upon an inner harmony, for both are statements that are totally devoid of meaning. But it is especially upon Goethe's view that color is accompanied by specific effects 'die sich unmittelbar an das Sittliche anschliessen,' and that while certain colors 'stimmen regsam, lebhaft, strebend, andere ruhig' (why not four distinguishable temperamental qualities, as there are four colors?), 'voll und ganz rein wirkt nur die Weisserregung.' 'Hence' (to give at last the details of the author's theory) the sensation conveyed by any single rod or cone is purely white, and a color-sensation is mediated by a combination of any two or of any three rods or cones. (Apparently it makes no difference whether a group of three is composed of rods and cones mixed up together or not.) But these retinal elements are not affected directly by light — that first reaches the cells of the pigment epithelium, passing through the thin plates of the retinal elements on its way. Here it effects a chemical change in the contents of these cells, and also in the pigment crystals which penetrate into the space between the rods and cones, and the products of this change act in turn upon the nerve ends within the visual elements. The idea that color can result from a fusion of colorless sensation-elements the author says has been suggested to him by the phenomenon which we call tone-*color* in sound — surely a very wild piece of analogizing!

It is singular how many ineptitudes a single theory of color is capable of containing; the one here reported on has them on every page. Space in this journal would be ill used in setting them all out, but it may be of some interest as a study in the working of a human mind, to indicate a few of them:

(*a*) When we get a tone-color by the fusing of a fundamental tone with its overtones, the things fused together are tones which already differ in quality (namely in pitch); but the three elementary sensations whose fusion is here to produce color are all alike — namely white. Three pieces of molasses candy fused together do not produce the taste of thoroughwort, nor of anything whatever other than molasses candy. No application of Fechner's law, nor of the formula

$$H = \sqrt{x^2 + y^2 + z^2},$$

will make anything but nonsense out of an idea like this.

(*b*) That it takes the working together of three visual elements (three cones, two cones and a rod, two rods and a cone or three rods) to produce the least extended sensation of color contradicts well-known facts — the firmness of vision in the fovea corresponds very closely with the fineness of structure of the retina, as has been well confirmed lately, among others, by Schoute. Minute points of light do not give different sensations as they fall upon one or another visual element in the rod-less region of the retina; hence there can be no difference in the functions of the several cones.

(*c*) More than this, it is fundamentally wrong to assume as essential to any visual sensation-quality the union of three contiguous visual elements. The spread-out-ness of the visual elements in the retina is the physiological correlative to the subjective feeling of extension; it cannot be at the same time the substratum for visual quality. In the ear, indeed, the successive efficient elements of structure are devoted to differences of quality, but that is not the case in the eye — there objective spatial extent gives us subjective spatial feeling.

(*d*) While there is no difference in the sensation produced by each of the three members of a group of visual elements, there is supposed to be a difference in their receptivity — the thickness of the plates in the end members is supposed to be so regulated as to make them pervious now to blue rays only, now to red and now to green. But this is just as it should not be — this is an arrangement for giving lying messages from the real world. Take the case of a small surface of a non-saturated purple; if it falls upon a happily chosen group of three visual elements, it will look as it should do — as if constituted of red and blue and a small portion of green. But if it changes its position a bit, it may hit a different group of three — say two red and one blue producing element; then it would look wholly saturated, and far more red than blue. And a wrong group of three it would

fall upon much more frequently than a right one. No correct and unvarying representation of nature could be obtained upon this scheme. This is, of course, an objection which applies to any three-fiber theory of color, but a three-fiber theory was long since given up by Helmholtz — hard as it seems to be for the knowledge of this fact to become widely distributed.

(*e*) A more fundamental difficulty still remains. When three fibers of a proper group are all equally affected, the sensation produced is white (including gray). A saturated blue means that a certain one of the three is strongly affected and the other two vary slightly. But when we come to the case of that same one being affected to the total exclusion of the other two, then we have again absolutely colorless sensation. But is not this pure nonsense? Was there ever a case of a theory which had been more purely non-thought-out than this?

(*f*) The beautifully fine structure of the retina in the fovea permits an exceedingly fine discrimination of parts in the visual field. But if a ray of light must go through the layer of cones and enter the pigment cells of the epithelium in order to produce there a chemical effect which has then to be reflected back into the cones, this firmness of structure would be thrown away; it is impossible that such a chemical effect should be reflected back into the single cone from which it came, and hence we should get only an enlarged and blurred image instead of a sharp one of any small point of light.

(*g*) As a fitting wind-up, we may mention that the author is guilty of a very singular lapse in a matter of elementary geometry — he thinks that if one cylinder is n times as long as another, it has a surface which is n^2 times as great. This is so curious a phenomenon that it is worth while to chronicle our author's very words. Speaking of the rods he says: *Ihre im Vergleich zu den benachbarten Zapfen mindestens um das Dreifache grössere Länge des Aussengliedes vergrössert natürlich die Oberfläche auf das Neunfache.* This is not so '*natürlich*' as Professor v. Oppolzer supposes, and such lapses are not calculated to inspire confidence in his long mathematical lucubrations. It is sad to see that the present paper is only a first installment of what is apparently to be a work of considerable length. It is a pity to waste precious pages of the *Zeitschrift* on such worthless matter, but it seems that even the acute responsible editor of that journal must sometimes nod.

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PSYCHOLOGICAL LITERATURE.

Human Personality and its Survival of Bodily Death. FREDERIC W. H. MYERS. London and New York, Longmans, Green and Co. 1903. Vol. I., pp. xlvi + 700; Vol. II., pp. xx + 660.

This posthumous work by the president of the Society for Psychical Research is called by its author a 'most imperfect text-book to a branch of research whose novelty and strangeness call urgently for some provisional systematization.' The text proper, which takes up scarcely one third of the space, is prefaced by a glossary and syllabi and is supplemented by elaborate appendices containing 'the mass of evidence already gathered together in the sixteen volumes of *Proceedings* and the nine volumes of the *Journal* of the S. P. R., in *Phantasms of the Living* and other books hereafter referred to, and in MS. collections.'

The book is put forward as an exposition rather than a proof and its line of argument is briefly an advance from the analysis of normal to the evidence for supernormal faculty ending with a discussion of the nature of the proof acquired as to the persistence of human personality after bodily death. The inquiry begins by discussing the subliminal structure, in disease or health, of those two familiar phases of human personality, ordinary waking and ordinary sleep. The next consideration is in what way the disintegration of personality by disease is met by its reintegration and purposive modification by hypnotism and self-suggestion. Dealing separately with the various groups of subliminal phenomena, the author treats of their mode of automatic manifestation and first of the sensory automatism which is the basis of hallucination, including phenomena claiming an origin outside the automatist's own mind. Finding that that origin is often to be sought in the minds of other living men, various forms of telepathy are reviewed. This telepathy is not confined to spirits still incarnate and evidence is offered that intercourse of similarly direct type can take place between discarnate and incarnate spirits.

In the introductory chapter is given this characteristic outline and justification of the work. Although it proposes to treat of the evolution of human personality, of faculties newly dawning and of a destiny greater than we know, yet there must needs be a detailed discussion

of certain modes of that personality's disintegration and decay. The extreme instances of such decay, actual imbecility or insanity, lie outside the author's province. But there are many cases where there is no actual insanity, probably no organic disease of the brain, but in which there are disturbances of personality which teach us of that complex structure or synergy which it is our object to upbuild or develop. Alterations of personality and hysterical phenomena are spontaneous experiments of the most instructive type. In hysteria, a vague range of phenomena called by a meaningless name, there is a contraction or effacement of the spectrum of consciousness, which leaves the hysteric occupying much the same position relatively to ourselves as our own supraliminal consciousness occupies relatively to our whole self. The essence of hysteria is an instability of the thresholds of consciousness and of voluntary movement, insomuch that many perceptions which should be fully conscious are for the time submerged, and many actions or motor syntheses which should be subject to waking will have sunk out of that will's control. Occasionally some faculties habitually submerged may rise into apprehension and there may thus be an analogy between hysteria and genius; genius consisting in an intensification of the conscious spectrum, hysteria in its dimming and interruption by dark belts of anæsthesia and aboulia, defect of perception and of will; genius consisting in the uprush of subliminal faculty, hysteria in the descent and disappearance of faculty which should be supraliminal into depths from which it cannot voluntarily be recalled.

An inspiration of genius is a subliminal uprush, an emergence into ordinary consciousness of ideas matured below the threshold. This view, given in Chapter III., differs from that of a current school of anthropologists who regard the man of genius as of an aberrant or even degenerate type. The alleged nervous disorder of men of genius illustrates the instability which in a rapidly changing species characterizes those very organs which are moving most decisively along the path of progress. If the word normal signifies such a combination of new with old powers as can at the present stage be affected without dangerous instability, so genius signifies a perturbation which masks evolution, the straining and disruption of the spiritual organism adapted to the earlier phase. The true analogue of the genius is not the criminal nor the lunatic, but the child. Chapter IV. deals with sleep as the alternating phase of personality adapted to maintain our existence in the spiritual environment, and to draw from thence the vitality of our physical organisms. Sleep is a relapse or reversion to

an earlier animal condition; a condition where the conscious part of the spectrum lay nearer to the red end; it also represents a stage of wider potentiality, where a longer spectrum is more faintly seen. Here there are traces of ultra-violet luminosity, faculties like telepathy and telæsthesia which form man's link with the spiritual world.

In Chapter IV. hypnotism is said to include all those empirical methods successful in inducing in man what is a development and concentration of his sleeping phase. Suggestion is a mere name for an appeal to subliminal faculty, but here we are not able either to predict or to explain its success or its failure. Long opposed or ignored by orthodox science, the psycho-physiological problems of hypnotism are quite unsolved and its profounder influences on personality have hardly yet been approached. Mesmerism and suggestion are different aspects of an influence which no theory fully explains. Suggestive therapeutics reproduces certain cures held of old as miraculous. As to religion, the influence which has been exerted upon the convert is intermediate between hypnotic artifice, dependent on trance-states for access to subliminal plasticity, and ordinary moral suasion, addressed primarily to ordinary waking reason. In somnambulism the influence is exercised by suggestion and self-suggestion on higher types of faculty, supernormal as well as normal. These nascent experiments give a pregnant hint that it may be in man's power to hasten his own evolution in ways previously unknown.

Chapter VI. deals with sensory automatism, especially the messages which the subliminal self sends up to the supraliminal in sensory form; the visions fashioned internally, but manifested not to the inward eye alone, the voices which repeat as though in audible tones the utterance of the self within. These hallucinations not only are consistent with health and sanity but also surpass the inspirations of genius in their manifestations of important faculty. Here we come upon experiments which prove telepathy, the transference of ideas and sensations from one mind to another without the agency of the recognized organs of sense. There is still a vast separation, unbridgeable at present by any hypothesis of ethereal vibrations or the like, between the smallest act of telepathic transmission and previous knowledge concerning matter and motion. There is no logical halting place between the first admission of supersensory faculty and the conclusion that such faculty is not generated from material elements, nor confined by mechanical limitations, but may survive and operate uninjured in a spiritual world. Among telepathic experiments there is the occasional power of some agent to project himself phantasmally.

Of spontaneous telepathic phenomena are apparitions of a distant person at moments of crisis, of coma, and of death.

In Chapter VII. is presented the supreme problem of the existence or non-existence of a spiritual world. The old conception of the ghost has received a new meaning from observations of phenomena occurring between living men. Phantasmal figures may bear a true relation to some distant person whose semblance is thus shown; wraiths of this kind correspond with death too often to leave the correspondence attributable to chance alone. There is no real break in the appearance of veridical phantasms or in their causation at the moment of bodily death, but there is evidence that the self-same living spirit is still operating. Telepathy looks like a law prevailing in the spiritual as well as in the material world, this is proved by the fact that those who communicated with us telepathically in this world do so from the other. Here the need of actual experiment is felt. There is a possibility of inducing a spiritual bearing and a spiritual picture-seeing or reading, and also a spiritually-guided writing and speech.

Chapter VIII. considers in what way motor automatism, the unwilled activity of hand or voice, may be used to convey messages which come to the automatist as though from without himself. Of course their apparent externality does not prove that they have not originated in submerged strata of the subject's mind. The messages from automatic writing among sane and healthy persons do not generally rise above the level of an incoherent dream. Sometimes they become veridical, convey a knowledge of actual facts of which the automatist has no previous information. This indicates some subliminal activity of the writer's own or some telepathic access to an external mind. This is independent of the question whether both minds, or only one, be still clad in flesh.

Chapter IX. deals with trance, possession, and ecstasy. Side by side with the automatism of arm and hand we must place the automatism of throat and tongue. Automatic utterance begins with mere incoherence but assumes a veridical character, with knowledge delivered from some subliminal stratum or some external mind. In trance the ordinary consciousness of the automatist seems suspended; this seems but the preparation for an occupation by an invading intelligence—by the surviving spirit of some recognizable departed friend. His friend then disposes of voice and hand almost as freely as though he were their legitimate owner. These trance-utterances can in part only be explained by telæsthesia and telepathy operating among actual scenes

and the minds of living men. Through the trance-phenomena of Mrs. Piper and Mr. Stainton Moses the evidence for communication with the spirits of identified deceased persons is established beyond serious attack. Eliminating conscious or unconscious fraud, self-suggestion, telepathy between the living and the like, we are forced to accept the messages as representing the continued identity of a former denizen of earth. Neither tradition or philosophy affords us any solid standpoint from which to criticise these messages. These evidences for the survival of human personality have never before approached so near to fulfilment. Especially in ecstasy do these messages from behind the veil help us to solve the relations of spiritual phenomena to space, time and the material world. The difficulties of communicating are such as might be inferred from the analogies between possession and alternating personalities, dreams and somnambulism, but the relations between mind and brain may be elucidated by the difficulties shown by the spirit in using the medium's brain.[1]

Chapter X. is an epilogue. It is here said that the evidence set forth in this book should prompt toward the ultimate achievement of scientific dominance in every department of human study, including, as never before, the realm of 'divine things.' Thus the conception of telepathy proves that the kinship between souls is more fundamental than their separation. Again whilst incarnate men have risen from savagery into intelligence, discarnate men have become more eager and able to communicate with earth. The response made in the past by human spirits of high type has been concordant in recognizing that a spiritual world underlies the material. This agreement is now supplemented by nascent discovery and revelation. Our evidence seems to indicate that the spiritual world is now just beginning to act systematically upon the material world.

These concluding words of Myers remind one of the manifesto of American spiritualism in 1848, the message to the Fox sisters that a reformation was going on in the spiritual world. It seems unfortunate that after all the efforts of the S. P. R. in studying borderland and residual phenomena their head should have brought forth such

[1] Telepathy indefinitely extends the range of an unembodied spirit's potential presence. Powers even more remote are retrocognition and precognition which drive us to postulate some coexistence of past and future in an eternal Now. As individual memory would serve to explain a large proportion of retrocognition, so individual forethought, a subliminal forethought, based often on profound organic facts not normally known to us, will explain a large proportion of precognition. Hence we are tempted to dream of a World-Soul whose future is as present to it as its past.

an historical paradox as this. The initial problem before the society was as to the nature and extent of any influence which may be exerted by one mind upon another apart from any generally recognized mode of perception. Assuming telepathy as proven and adding to it the more occult powers of telæsthesia and telekinesis Myers has based them all upon the subliminal self. But in sinking the foundations so deep the structure itself has suffered distortion. In point of style the outward embellishments are irreproachable, but from the standpoint of normal psychology it is a well-nigh hopeless task to attempt to straighten the building. Beginning with the contrasted views of personality, the old fashioned view of a single unitary personality and the modern view that the self is a coördination, the author's contention as to the abiding unity of the ego is that it consists of ultimate infinitesimal psychic elements which withstand the shock of detah. This proposition is subsumed under another equally mystical, namely that there is a more comprehensive consciousness, a profounder faculty which reasserts itself after death. To prove that this transcendental self survives the empirical self, the nerve-tract theory is brushed aside and there is offered in its stead a vague statement as to subliminal uprushes, the impulses or communications which reach our emergent from our submerged self. At this point a logical difficulty is anticipated; the theory of the subliminal self, it is said, need not be pushed so far as altogether to negative spirit-intervention, because the faculties of telepathy and telæsthesia suggest either incalculable extension of our mental powers, or else the influence upon us of minds freer and less trammelled than our own.

Discussing, in the next place, disintegrations of personality, the author employs the contradiction of an unconscious consciousness in speaking of the unreachable subliminal reminiscences which give the signals for hysterical attacks. The statement that the latter are caused as well as cured by the subliminal self is evidently not seen to be inconsistent with the later optimistic assumption that the subliminal activities are beneficial. Furthermore, in the failure to relegate terrifying dreams, epileptic mania and the deep self-absorption of melancholia to abnormalities of the nervous system, Myers' 'scheme of vital faculty' is indeed such as no physiologist would care to sanction. This neglect of cerebral conditions is matched by the failure to attempt to explain alterations of personality in terms of more ordinary psychic functioning. In multiplex personality the neurosis plays its part; so, for one thing, does the imagination in this dramatic sundering of the self.

The treatment of the inspiration of genius as due to subliminal ideation leaves the subject as much in the dark as ever. The achievements of arithmetical prodigies may be offered in support of the marvellousness of subconscious activities, but the rather dubious tales of mathematical solutions in dreams are given without reference to the preceding waking efforts to solve the problem. If the subnormal fails to explain normal ratiocination, equally little light is thrown on the phenomena of memory by emphasis on the unbroken continuity of the subliminal recollection. A physiological definition of sleep has in truth never yet been achieved, but the cases of hypnotic sleep here cited show that there is persistence of suggested ideas in a dim way into the conscious state. Why the awakened subject remembers these products of 'subliminal mentation' is, in essence, no more mysterious than that he remembers at all. A further overburdening of the subconscious and a further disregard of the physiological side is illustrated by the contention regarding the regenerative and vivifying power which the subliminal self habitually manifests in sleep. The meaning of this magical formula is further explained in the statement that we are living a life in two worlds. The working personality is adapted to the needs of earthly life; the personality of sleep maintains the fundamental connection between the organism and the spiritual world by supplying it with spiritual energy during sleep, and itself develops by the exercise of its own spiritual faculties.

The chapter on hypnotism is perhaps the most interesting in the book. Myers openly takes issue with the Nancy School and asserts that suggestion is a premature attempt to simplify supernormal communications. But in attributing suggestion to aura and emanations the author is guilty of a theory equally retrogressive. Recent attempts to place suggestion under the general laws of association are stultified and there is a return to the days of odyllic forces. Without giving an historical survey of hypnotism, which manifests a decided trend toward explanations in terms of normal psychology, Myers claims that suggestion is not comparable with supraliminal sensation and endeavor. Its real efficiency, he says, lies among subliminal processess, as an empirical facilitation of our absorption of spiritual energy or acquisition of directive force from a metethereal environment. The grounds for this mystical conclusion may be shown to be quite self-contradictory. To give them as they are presented: Hypnotism in animals is laid to catalepsy and paralysis from fright but in man to the subliminal self. Metallæsthesia is attributable to obscure chemical reactions and also the sensitiveness of the central senso-

rium. The perception of the lapse of time in birds is due to the panæsthesia of the primal germ, in man to the secondary personality. Stigmatization on the right side, while the stimulus is on the left, is attributed not to decussation but to the subnormal intelligence presiding over the organic suggestions. Crustacean recuperativeness is possessed by the lobster because of self-suggestion; it is lacking in the mammal because of the inhibition of hysterical self-suggestion. In vesication hypnotic suggestion awakens the dormant plasticity not blindly but with intelligent caprice. Post-hypnotic suggestions are obeyed blindly and also manifest the intelligence of subliminal mentation; they may lead to kleptomania and also develop the higher sense of propriety. In rapport the hypnotized subject knows supernormally the superficial sensations of his hypnotizer, but this community of sensation needs education and development. In subliminal states like trance there is greater responsiveness to spiritual appeal, yet it is the lower organic centers which are under more control. Finally, in the somnambulic state we are introduced to two subliminal powers apparently quite disparate, the sanative which modifies the body, the talæsthetic which quits the body. In the face of these inconsistencies and incoherencies Myers still claims to have placed suggestion in truer relation to other forms of external suasion and intellectual will than those who compare it with supraliminal suasion and endeavor.

In the chapters on sensory and motor automatisms there again appears the writer's curious aversion to the correlation of the alleged subconscious with ordinary psychoses, for example rapport as a form of concentrated attention, voluntarily limited to the operator. There is also exhibited his disinclination to recognize mere neural activities. Thus in citing experiments in which by 'silent willing' a finger becomes cold much is said about supernormal effluence, nothing of peculiarities of vaso-motor action. So in regard to glossolaly, automatic writing and inspirational speaking, there is much about messages from the subliminal, nothing about cerebral decentralization.

To convert the author's proposition — it is ordinary psychology which has become the excrescence on the subliminal life. When hypnagogic images are called vaguely typical this is laid to subliminal generalization not to suspension of voluntary attention. So many illusions of sense are counted veridical impressions or pictures and not mere subjective fancies. In crystal visions normal and supernormal knowledge and imaginings are considered to be strangely mingled. Memory, dream, telepathy, telæsthesia, retrocognition, precognition, all are there. In trying to account for these occult powers Myers is

naturally averse to physical explanations and tests. Sir W. Crooke's vibration theory of telepathy is discarded because incompatible with the phenomenon that the percipient's mind modifies the picture despatched from the agent. There is of course no recourse to the alternative explanation of everyday psychology, that the so-called telepathic message, obtained from vague sensory hints such as pressure and unconscious whispering, undergoes an inevitable subjective generalization. Again it is asserted that telæsthetic visions may show great laxity of time relations, because if they are not synchronous they are either precognitions or retrocognitions. This convenient method of evading the natural by offering supernatural alternatives is further exemplified in clairvoyant visions which are symbolical, not because there is no such thing as clairvoyance, but because they are not located by the observer in ordinary three-dimensional space.

The convenience of having several lines to catch your fish is best illustrated in the chapter on phantasms of the dead. Discarding a previous view of the essential unity of the self through a subliminal substratum, the problem now starts from a root-conception of the dissociability of the self, that segments of personality can operate in apparent separation from the organism. At this point the author appears to recognize the hypothetical character of his whole argument. He puts it in this syllogistic form: If we have once got a man's *thought* operating apart from his body there is no obvious halting-place on *his* side till we come to 'possession' by a departed spirit, and there is no obvious halting-place on *my* side till we come to 'traveling clairvoyance,' with a corresponding visibility of my own phantasm to other persons in the scenes which I spiritually visit.

The rest of the work is confessedly a palæolithic psychology and Myers' reversion to the beliefs of the Stone Age in ghosts, haunted places, and heaven-sent dreams is supported merely by a negative: 'What definite reason do I know why this should *not* be true?' Against this constitutional will to believe the only thing to do is to separate the elements of fact and fiction in the psychology of the psychic researcher. The census of hallucinations, the citation of hundreds of cases of externalized apparitions is not a complete answer to the danger of illusions of memory, creating or magnifying the interesting coincidences, for these illusions are matters not of general tabulation but of individual introspection. The incoherence of planchette messages is scarcely to be taken as due to the jostling of the spirits, when we have the same incoherence in the automatograph due to uncontrolled muscular action. This persistent turning toward sub-

terranean explanations is again illustrated in the citation of Professor Flournoy's study of a case of somnambulism with glossolaly. After a painstaking *Quellensucht* for the elements of Mlle. Smith's cryptic Martian language, Myers concludes that the knowledge of Sanscrit betrayed therein was not derived from a Sanscrit grammar known to be in the room in which the séances were held. On the contrary it was clairvoyantly acquired by the subliminal self.

In conclusion, by putting forward the Moses-Piper group of trance phenomena as evidence for spirit possession, the author's language, to use his own simile, suggests the medicine-man's wigwam rather than the study of the white philosopher. This unscientific attitude is unfortunately true. Here for once there is failure even to consider the alternative explanation. In the case of Stainton Moses, it does not seem to have occurred to Myers, as it has to his colleague Podmore, that there was deliberate fraud in the twilight materialization of a phosphorescent demon. In the case of Mrs. Piper there is no rebuttal of Professor Lodge's surmise that on the part of the Phinuit 'control' there was a system of ingenious fishing: the utilization of trivial indications, of every intimation, audible, tactile, muscular, and of little shades of manner too indefinable to name.

The epilogue to these volumes reads like the *Philosophy of Spiritual Intercourse* of Andrew Jackson Davis, the 'Poughkeepsie seer.' In the one psychic research is urged as a duty much as in the other spiritualism was urged as a religion. But this study of 'responses to stimuli spiritually controlled' can scarcely be considered a 'profound cosmic thesis for scientific proof' so long as the S.P.R. requests posthumous letters, containing test sentences, as proofs of spiritual survival, and so long as it depends on the utterances of a medium who mistakes Baby Marian for Baby Timmins.

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Agnosticism. Robert Flint. New York, Scribners. 1903. Pp. 664.

The present volume forms a part of a general system of natural theology which embraces also the well-known writings of Professor Flint on *Theism* and *Anti-Theistic Theories*. The work on *Agnosticism* is a continuation of the latter, and is conceived and carried out in the same spirit of historical research and candid criticism which characterizes all of Professor Flint's philosophical discussions. As a

part of a system of natural theology, religious agnosticism is the special phase of the subject which it is the purpose of this book to examine and combat. There is, however, a full recognition of the other phases of agnosticism, such as that concerning cognition in general or concerning the world, or self. While these phases are discussed from the point of view concerning the separate spheres to which they specifically relate, nevertheless Professor Flint has always in mind their ultimate bearing upon religious agnosticism. There is from the outset of the discussion a clear cut line of distinction drawn between agnosticism proper, and the many loose and misleading senses in which the term is used. Thus agnosticism is sharply differentiated from honesty in investigation, from nescience, from atheism, and from such a view as Leslie Stephen's that agnosticism is the opposite of gnosticism.

Agnosticism with the author, means 'the theory of the nature and limits of human intelligence which questions either the certainty of all knowledge and the veracity of every mental power, or the certainty of some particular kind of knowledge, and the veracity of some particular mental power or powers' (p. 22). Again in a similar vein he defines agnosticism as that form of skepticism which is doubt or disbelief 'resting on the supposition that what are really powers of the human mind are untrustworthy; that what are actually normal perceptions, natural or even necessary laws and legitimate processes are not to be depended upon' (p. 23). In this definition, the author has rendered to the philosophical world a double service. He has extricated the term agnosticism from a confused mass of conflicting connotation, and unwarrantable implication. And also, he has emphasized the necessity of grounding any satisfactory defense of theism upon a sound epistemological basis. He is, moreover, out of all sympathy with the so-called 'agnosticism of piety' which exalts pious feeling at the expense of reason. The main lines of Professor Flint's criticism of the agnostic position may be briefly indicated as follows:

Epistemology as the theory of knowledge should be the complete theory of knowledge. It is not entitled to lay down as limits of knowledge what are merely limits of a specific kind of knowledge. "Whatever claims to be knowledge should have its claims fairly examined and should not be set aside as pseudo-science in misplaced confidence on any superficial generalization or dogmatic assumption as to what is and what is not knowledge. Hence epistemological theory cannot of itself warrant us to pronounce physiology, for example, a real science, and psychology a pretended one, sense perception a faculty of knowledge, but apprehension of the divine an illu

sion, phenomena within and noumena without the sphere of cognition, etc." (p. 339). He criticises the agnostic position that because there is not complete knowledge therefore the knowledge which the mind does possess is of no value, and maintains that all knowledge is progressive, that while the knowledge of God is incomplete, it is nevertheless a knowledge which is growing from age to age, and forms so integral a part of the knowledge of the world and of ourselves that the advancement of science, the progress of history, the more exact investigations of the human mind, all contribute to a deeper knowledge of the divine power of which they are the various manifestations.

Against the doctrines of Hamilton, Mansel and Spencer, the author contends that the idea of the absolute is not involved in absurd contradictions, is not the wholly indeterminate, not that which is out of relation to all things, not the unknowable, but the ground of all relationship and the essential content of all knowledge.

He contends, moreover, against the Kantian diremption of phenomena and noumena, of thought and of being that, so far as it is possible to know at all, we know through 'ideas which are absolute and noumenal in the only intelligible and in a very real and important sense' (p. 645). They condition experience and are not conditioned by experience. He maintains, moreover, that Kant's criticism of the theistic proofs has modified their statement but not their underlying and essential principles, and that Kant's moral argument for the being of God to which his practical reason so strongly assented, is in principle similar to the cosmological, and the physico-theological arguments, that, in short, no sharp line or distinction should be drawn between the pure reason and the practical reason. Throughout this discussion Professor Flint has treated his opponent's views with fairness, and with a sympathetic appreciation of their positions. His historical survey of the history of agnosticism is very complete, and provides an excellent source for convenient reference. The arrangement of his material in a different manner would have rendered his exposition clearer and more effective. For instance, in the earlier part of the work, he gives an historical sketch of agnosticism in general, and in the latter part of that particular form of agnosticism which is anti-religious. Again, in one chapter he treats of agnosticism as to God, and in a later chapter of agnosticism as to knowledge of God. The distinction is not clear and is not significant. Moreover he refers to Huxley's agnosticism as equivalent to 'a spirit of intellectual honesty in investigation' (p. 42), which definition of course can raise no dis-

sent. Huxley's agnosticism, however, was more than a plea for absolute honesty in investigation, it affirmed also that an intellectual honesty in investigation could never carry one beyond the sphere of an experience resting solely upon data of the senses, and that the only proof which could appeal to an honest mind is that which swings clear of all metaphysical presuppositions and dogmatic assertions. Huxley's name is so identified with the term agnosticism that the precise connotation which he gave to it should be clearly recognized.

Professor Flint's plan for his system of natural theology calls for further volumes on the idea of God as disclosed by nature, mind and history, together with a tracing of the use and development of the idea of God, and the history of theistic speculation (p. 640). We sincerely hope that his strength and years may be sufficient for the completion of a task whose accomplishment so far has placed the philosophical world under so great indebtedness to him.

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Le Caractère. P. MALAPERT. (Bibliothèque Internationale de Psychologie Expérimentale.) Paris, Octave Doin. 1902. Pp. 305.

Noteworthy attempts have been made within the past few years to give form and substance to the idea of a science of character first proposed by J. S. Mill under the term ethology and variously designated by other writers as characterology and as individual or, again, differential psychology. The result has been the accumulation of a large amount of material bearing on the problems involved and the development of numerous conflicting, but suggestive, points of view. It may even be said that the science has been founded, at least a beginning had been made, which would seem to justify reasonable expectations. The work of construction has been undertaken mainly by the French. The work is relatively so new, while yet so much work has already been done, that it is both possible and profitable to survey and estimate the whole of it. It is this task which M. Malapert has set himself, and which he has executed with admirable skill, in the volume before us.

Mill's conception of a science of character has been appreciably modified in the course of the discussion. Mill conceived the science as a counterpart of the art of education, its object, or at any rate its principal object, being to deduce from the general laws of psychology, assumed as known, the sort of character, national, collective or indi-

vidual, which would result from a given set of physical and moral conditions. Ethology, in brief, was to be a science of the laws governing the formation of character, its method, in Mill's view, being mainly deductive. In the newer conception, the first object of the science is to define and classify the various types of character. It is only in the second place that it investigates the causes and laws of their genesis and transformation. The emphasis put upon the former object by recent writers has accordingly tended to thrust into the background the deductive method approved of Mill and to the employment of clinical and experimental methods of observation. The main stress is still on individual ethology, though various attempts have been made to deal also with the characters of social and national groups.

It should be observed that the term 'character' in this connection is used in the large sense of the system of psychical dispositions by which an individual is characterized. What is meant by a 'type' of character is not so easily stated. It is held, however, in general, to be constituted by the relative preponderance, force, form, direction, vivacity and mutual relations of mental elements and functions. The science has mainly to do with the precise analysis and synthesis of these elements. But the terms employed are all more or less vague and the problem is extremely complex. The psychical individual seems to elude precise analysis, psychical individuality, constituted as it is by a unique synthesis, appears as a positive limit to science. Nevertheless psychical individuals do represent — who can doubt it? — more or less distinct types of character, corresponding somewhat to the genera, species and varieties of general biology. And the determination of these types presents itself as a possible problem. It is only when we come to examine the attempts that have been made to solve the problem that the vagueness of the conceptions and the arbitrariness of the principles of classification adopted by different writers make evident how great and difficult the problem really is. All this appears frankly confessed in the pages of M. Malapert. The successive chapters treating of the object and method of the science, of the factors of character, of the metaphysical theories, the theories of temperament and the psychological theories, then the important chapter on the classification of characters, finally a chapter treating of abnormal and morbid characters, all show a conflict of opinion that makes the task seem well-nigh hopeless. And yet it is not perhaps altogether hopeless. In a concluding section M. Malapert enumerates several circumstances which serve to tone down the apparent conflict in opinion, *e. g.*, the lack of precision in the psychological vo-

cabulary and the tendency on the part of writers themselves to exaggerate their differences. He also remarks, as a sign of progress, on the general approach to at least a negative agreement in the elimination of certain problems and points of view, *e. g.*, metaphysical speculations concerning the nature and origin of the individual and, again, the theory of temperament as a point of departure for the study of character. He points further to the large amount of positive material bearing on the problem obtained in numerous researches into the variations of mental processes in individuals. And although little has been done which finds general acceptance in the way of determining the relations of the different mental processes in individuals, little to give definiteness to the vague conception of preponderance of elements and to fix the character and limits of their mutual influence, we have, he thinks, even in this regard the tentatives which can alone be expected in so new a science.

The student of human nature will justly estimate highly the value of a monograph such as this, which serves as a trustworthy guide to the history and problems of a branch of psychology so new, so attractive, so large in possibilities. M. Malapert is very clear and objective in his expositions and singularly fair, temperate and broad-minded in his criticisms. He is scarcely, if at all, less conscious of the limitations of his own views than he is of those of others. Thus, after giving his own classification of characters into apathetic, affective, intellectual, active, balanced (*tempérés*) and voluntary, he indicates quite plainly that he does not consider it one which can claim to be perfectly natural, based on a principle recognized by all (p. 269). In fact it rests on a four-fold division of the mental faculties into sensibility, intellect, activity and will (p. 209) which may very well be questioned. For while it is perfectly true, as the author, following Fouillée, contends, in criticising Ribot's division of faculty into feeling and action, that intellect is a human characteristic, and while voluntary activity, in the strict sense, is, as the author justly maintains, obviously distinct from mere motor activity, the sense in which the term 'activity' is used in the above division, neither intellect nor will can perhaps properly be regarded, from the psychological point of view, as fundamental, certainly not as elementary mental functions. Voluntary activity, in the strict sense, would seem rather to be but a highly developed complex organization of ideal tendencies or conations and the intellectual life itself but a form and expression of the fundamental sensibility in which it is found constantly embodied. But if this is so, if feeling and the conative aspect of feeling form the

sole ultimate empirical basis of consciousness, and if a classification of types of character is to be based on an analysis of ultimate functions, the classification adopted by M. Malapert can scarcely be maintained. But while it would be interesting to see a classification of types of character derived from the above principle not open to the objections brought against Ribot's classification (p. 251) based on this principle in a somewhat different form of statement, it must be admitted that the principle itself, like every other, is still too little developed and too far from general acceptance to form the foundation of a science.

H. N. GARDINER.

SMITH COLLEGE.

Studies in the Cartesian Philosophy. By NORMAN SMITH. London and New York, Macmillan.

This is a somewhat notable contribution to the literature on Descartes and his influence on subsequent thinking. The aim of the work is critical rather than expository, but there is no lack of exposition of a very clear and illuminating kind. In fact the best informed student of Descartes will come from the perusal of this book with renewed insight; or at least with sharpened perceptions. The quality of Professor Smith's work cannot, in fact, be too highly commended. His faculty of analysis and his power of defining and stating issues clearly, is quite remarkable and his criticisms are always trenchant and mostly just. The discriminating way in which he traces the Cartesian influence through Descartes' successors down to Kant is quite masterly and generally convincing. At the basis of Professor Smith's criticism is a distinction between Descartes' contributions to natural science and his metaphysics. Of the former he says: 'In a more adequate manner than even Galileo or Bacon, Descartes formulated the methods and defined the ideals of modern science.' But in metaphysics he finds him still in bondage to scholastic abstractions and to what he calls Plato's mystical idealism. The author deals with the system of Descartes under two general heads, his *Method* and his *Metaphysics*. The former is found to be mistaken not so much *qua* mathematical, as because it involves a false conception of mathematics itself, one that having first created a dualism between perceptions and conceptions and elevated the latter into absolutes, identifies mathematical method with an analytic deduction of the content of these conceptual terms. It is easy for the author to show that such a method results in a pure rationalism that eliminates sense perception altogether. Moreover, it results in a dualism between algebra and

geometry, inasmuch as the latter clearly involves space which the author claims is no conception but a concrete reality revealed in perception. The one merit of Descartes' method is its insistence on clearness and distinctness as essential to validity.

In his discussion of the Cartesian metaphysics the author points out how the conceptualism of Descartes leads logically to pure rationalism which would exclude all perceptual and volitional elements from knowledge. But the dualism of matter and mind breaks the rationalistic flight as it were in mid air. We reach the existence of self, not intuitively, but through the immediate implication of the *cogito* But we have no such resource in the case of the external world and the completely alien nature of matter. Here Descartes' representative theory and with it the unimpeded flight of his rationalism breaks down. In order to guarantee the truth of our ideas of the objective world or non-ego, Descartes is compelled to bring in his spiritualism, including the agency of God, and the sensational and volitional activities of the soul. The author shows very clearly how the spiritualism of the system drifts inevitably to *Occasionalism* although Descartes fights against that consummation.

The most interesting chapters in Professor Smith's book are those in which he follows the fortunes of the Cartesian rationalism through the thought of Descartes' successors of both the rationalistic and empirical schools. We, of course, expect to find Spinoza and Leibnitz, reputed rationalists, more or less under Cartesian influence. And we have become accustomed to regarding Locke as a sort of halfway thinker in whose work a certain amount of rationalistic lumber exists as a mere uncritical survival. Our author contends, however, and we think successfully, that Locke was fully as much a rationalist as an empiricist in his own method and doctrines. But that the thorough-going hater of rationalism, David Hume, should himself be to some extent its victim is a fate that we would not invoke for our worst enemy. Nevertheless, the great sceptic stands convicted and Professor Smith is able to distinguish the actual Hume from the Hume history would have presented to us, had he been wholly free from traditional Cartesian prepossessions.

It is only in Kant, the author says, that we reach a conception that has finally emancipated itself from rationalistic prejudices. The chapter on Kant is in some respects the most unsatisfactory in the book. It is long enough for the author to tell us that in Kant's Copernican revolution and critical idealism he has achieved for the first time the hypothetical method and pure phenomenalism of modern

science. It is too short, however, to inform us whether the author thinks the significance of Kant is exhausted in this result. There is a point of view, which the author has stated clearly from which pure phenomenalism or positivism could be considered a logical outcome of Kantism. But if the claim were made exclusive so as to shut out other points of view as mistaken, then Hegel might put in a demurrer. Those who believe that Kant may be reconstructed from the point of view of things in themselves might also be impelled to file an objection. Nevertheless, we have reason to thank the author for the Cartesian clearness and distinctness which make his own work delightful reading, as well as for the masterly character of his discussion as a whole. A feature of the book is its beautifully clear typography, especially that of the footnotes which the eye can take in at a glance.

ALEXANDER T. ORMOND.

PRINCETON UNIVERSITY.

Hegel's Logic, An Essay in Interpretation. JOHN GRIER HIBBEN. New York, Scribners. 1902. Pp. x + 373.

We are glad of this addition to the books, already become a row on the library shelf, which are entitled Hegel's Logic. Although coming last it will serve for many as a key to its predecessors. To say the least it is intelligible and easy to read, two factors which will not lay Professor Hibben as open to the charge of infidelity toward Hegel as some might think.

In bringing Hegel up to date there is of course a tendency to substitute phrases which shall make us less prone to disagreement and make him more in line with the emphasis of to-day. For instance, on page 18 Professor Hibben asserts as Hegel's position that 'Reason has two sides — a thought side and a force side, a rational and a dynamic essence — and these two are one.' And again on page 4: 'The creative and sustaining source of the universe is thought force.' To us the phrase 'thought force' speaks of Fouillée's *Idées Forces*, of Ravaisson's realistic spiritualism, of modern panpsychism and of the whole American emphasis on will; an emphasis perhaps not inconsistent with, yet certainly not to be gathered from Hegel's own statement of his position. Professor Hibben has on his side the possibility that had Hegel foreseen the direction which criticism would take, his emphasis would have met it in about this way.

The book is and perhaps purports to be a summary with explanatory notes woven in, in readable form, of Hegel's shorter Logic as found in Part I. of the 'Encyclopedia of Sciences,' which was published

in 1817. This part I. in turn is an abbreviated and annotated edition of the two-volume work entitled the 'Science of Logic,' 1812–1816. This larger Logic is the one which Professor W. T. Harris took as the basis for his Hegel's Logic in Grigg's Philosophical Classics.

Of Professor Wallace's two volumes entitled Hegel's Logic, the first is a translation with notes of the shorter Logic of the 'Encyclopedia of Sciences.' In the second volume are prolegomena to the whole of Hegel's philosophy as much as to the Logic.

Dr. Baillie's recent book on Hegel's Logic is a general introduction to Hegel's system not at all adapted however to give one a first insight into Hegel.

For this Professor Hibben's book is specially adapted and the glossary of philosophical terms in the appendix would indicate that he intended the book to be used as an accompaniment to the reading of Hegel in the original. Lucidity has apparently been his chief desire and therefore he is to be pardoned for his boldness in always trying to make Hegel say something intelligible. It is a greater injustice to Hegel to expound him so delicately that he remains unread than to run the risk of misinterpretation or underinterpretation while making clear his importance to us. Professor Hibben's book will do good service not only as an introduction but also as a stimulus to the reading of Hegel.

GEO. R. MONTGOMERY.

YALE UNIVERSITY.

Heredity and Social Progress. SIMON N. PATTEN. New York, Macmillans. 1903. Pp. vii + 214.

Readers of Professor Patten's former works will be prepared for audacious generalizations, but this book seems to offer a maximum of hypotheses with a minimum of supporting evidence. The fundamental thesis presented is that progress starts from a surplus rather than from a deficit, as is assumed by 'current biology and classical economics.' The problem as stated in the terms of economics is: How can the social surplus, wrung from nature by conscious effort in the face of diminishing natural returns, be transformed into mental traits that abide and become the basis of subsequent progress? Stated in biological terms, the problem is: How can acquired characters become natural?

The method is an attempted parallel between biological and psychical processes which will be likely to impress the psychologist

as based on analogies of the most superficial kind. Thus: memory implies related parts; it corresponds, therefore, to growth in the structure. Visualization is the mental struggle for complementary ideas; it corresponds, therefore, to a physical process of regeneration in a disrupted structure. "Memory and visualization are thus at opposite poles of thought."

Perhaps the most startling bit of psychology is found in the chapter on 'The Inner Organs of Expression.' The general thesis of the chapter is to the effect that, as there has been a parallel development between the outer body on the one hand, and an inner neural body on the other, we may expect the inner as well as the outer to retain some traits characteristic of more primitive stages, and these may become the organs for acquired characters. The physical basis of reasoning is sought for along this line: Reasoning is a process of rejecting dissimilars and accepting a similar. Now recoil from the dissimilar and the acceptance of the similar is a tendency of unicellular organisms, like the amœba. Reasoning has no new element. Here consciousness shows its elementary and primitive character, whereas digestion demands a hundred independent reactions. The naïveté with which the process of reasoning is here conceived is scarcely to be matched this side of the earliest efforts of the Greeks, and while there are many problems proposed throughout the book which challenge attention, the author's psychology is certain to awaken distrust as to his solutions. If they are right it is not because of his processes.

J. H. TUFTS.

UNIVERSITY OF CHICAGO.

HEARING.

Der Tonvariator. L. WILLIAM STERN. Zeitschrift f. Psych. u. Physiol. d. Sinnesorgane, 30 (5–6), 1902, pp. 422–432.

The instrument is a blown bottle, made of metal, without a bottom, but with a movable piston in its stead. The piston is moved by means of a spiral in such a way that the increase or decrease of the vibration rate is proportional to the angular velocity of the spiral. More than one bottle may be combined on the same stand. The range of a single bottle is usually about an octave; less, if the pitch of the bottle is very low. The instrument is doubtless a very useful source of sound for many purposes.

MAX MEYER.

UNIVERSITY OF MISSOURI.

Ueber binaurale Schwebungen. P. Rostosky. Philos. Studien, 19 (Festschrift, I.), 1902, pp. 557–598.

The author's problem is this: Are there binaural beats of central origin? The experiments made to solve this problem are the following: The tone of an electromagnetic tuning fork was conducted through two brass tubes of equal length to the two ears of the observer, sitting in another room. When the intensity of the tone acting upon one ear was made less than on the other, the tone perception was localized on the side of the stronger stimulus. When the intensity was the same, but the phase was altered, a very peculiar change in the subjective localization was observed. Now, since the phase as such is imperceptible, the author attempts to reduce theoretically the case of difference of phase to a case of difference of intensity by assuming that the vibrations acting upon the left ear do not send an exactly corresponding nervous process to the center, but that they suffer a certain interference from the vibrations acting upon the right ear. (And the same for the other ear.) He develops the mathematical theory of this function in all details, demonstrating that the result of the above assumption must be exactly those changes in the relative intensity of the sensation of either ear, which agree with the peculiar changes of the subjective localization observed in the experiments. It is, therefore, proved that the vibrations set up within one ear suffer interference from the vibrations set up within the other ear; and the amount of this interference is accurately expressed by a formula. The question to be answered now is this: Does this interference occur within the peripheral organs, or do the nervous processes coming from both ears, before arriving at the cortex, pass through two lower nerve-centers in such a way that the greater part of the nervous process coming from one ear passes through the lower center of the same side, a fraction, however, to the lower center of the other side, causing thus the interference which the author has mathematically analyzed? The latter explanation of the interference is possible only if we assume that the nervous impulses set up by the objective vibrations are of an oscillatory nature. The author rejects this assumption as unfounded and improbable, and adopts the assumption that the interference occurs in the peripheral organs. The vibrations are mechanically conducted from one ear to the other by way of the cranium. The physical constants in this case agree sufficiently well with the constants assumed in the mathematical analysis. The theory of central interference is, therefore, not only improbable, but entirely superfluous. The objections to this explanation by mechanical conduction raised by

different writers are not valid. The method of observing the localization is much more sensitive and exact than any direct method of observing the relative intensity of the sensation in either ear, and its results are, therefore, of more weight. It can be applied with the same result to stimuli, which, on a single ear, are below the threshold.

MAX MEYER.

UNIVERSITY OF MISSOURI.

EXPERIMENTAL.

An Analytic Study of the Memory Image and the Process of Judgment in the Discrimination of Clangs and Tones. G. M. WHIPPLE. Amer. Journal of Psychology, 13 (2), April, 1902, pp. 219–268.

The author continues his previously published investigation into the discrimination of tones, by using tones of continuously changing pitch instead of discrete tones. The observer reacted when he thought that the second tone had reached a pitch equal to the standard pitch. Between the first and second tone was a time interval of either ten or forty seconds. Generally the observers reacted too early, and the more so the longer the time of change (proportional to the pitch difference) was. The author mentions a great number of interesting introspections of the observers with respect to the early reaction and other facts. Some observers have a distinct emotional preference in regard to the direction of the variable stimulus; they prefer to listen to a rising, or to a falling tone. One of the observers (who was less musical) used other characteristics more than the pitch itself: visual and temperature sensations in particular. In a series with a time interval of forty seconds the quantitative results were much more irregular than with a time interval of ten seconds. The loss of the auditory image did not make the judgment impossible, but the presence of the image afforded greater assurance in the reaction. A third series consisted of experiments with knowledge. The general effect of the knowledge of the coming position of the variable tone gave a feeling of security, did away with the momentary perplexity. A second effect is the presence of an anticipatory image of the variable stimulus. In a fourth series of experiments with a time interval of ten seconds the memory image was eliminated, so far as possible, by distraction set up by odors. Distraction, like a long time interval, lessens assurance. The expectation error is very materially broken up.

Some special tests were made on the unmusical observer, who used visualization. Her drawings of the movement of the variable tone agreed very well with the actual change.

In regard to the nature and course of the memory image the author draws these conclusions: The auditory memory image is but one part of a complex structure which represents the original experience. The memory image of a tone is not a tonal memory image; it is that and much more. The auditory image proper attains its maximal excellence about two seconds after the stimulus. It is in a very unsatisfactory condition at 40 seconds. The other constituents of the memory image do not necessarily follow the course of the auditory core; they may be serviceable for purposes of discrimination when the auditory image has disappeared entirely. Practice increases the serviceability of the image. The task of actively holding the image very soon develops a habit of imaging; the image, that is, of itself becomes so insistent that, when exclusion of the image is desired, very active attention to naturally powerful distractors is necessary. The presence of the auditory image is not necessary to the recognition of either difference or equality.

MAX MAYER.

UNIVERSITY OF MISSOURI.

Ueber Vertheilung und Empfindlichkeit der Tastpunkte. FRIEDRICH KIESOW. Philos. Stud., B. 19, 260–309.

This is a psycho-physiological study of 'touch spots' with special reference to the relation of tactile organs to hairs. It is a continuation of investigations begun at an earlier date in company with von Frey (*Zeitschr. f. Psych.*, 20, 126). Aside from a brief critical review of work done by Blix, Goldscheider and others the work is valuable chiefly for the large amount of carefully collected data. It contains twenty-seven tables, besides many columns of figures not included in the tables. For the purpose of close study the surface of the skin on different parts of the body was marked off into small areas, and a lens was used to locate the points, which were carefully marked when found. Pure tactile points not connected with hairs, were found on haired surfaces, but in comparatively small numbers. Individual differences are considerable, as might be expected from the great difference in the number and arrangement of hairs on different persons.

Die Ebbinghaus'sche Combinationsmethode. E. WIERSMA. Zeitschr. f. Psych. u. Phys. der Sinnesorg., B. 30, Heft 3, 196–222.

In this article Dr. Wiersma reports some investigations carried on under what seemed to be very favorable conditions for testing the so-called Combinationsmethode of Ebbinghaus. He used the same methods of investigation and calculated his results in the same manner as did Ebbinghaus (*Zeitschr. f. Psych.*, 13, 401). The experiments were made on the students of two schools, one for each sex in which admission was by competitive examination. This, he thinks, secures students of about the same ability when they enter school. It may be said, parenthetically, that no significant sex difference was noticed. The results from these students were compared with those from another school in which no unusual entrance conditions existed. By comparison of the results obtained from the lower and higher classes in the schools it seems possible, in some degree, to distinguish between the capability due to endowment (*Begabung*) and that due to development (*Entwickelung*). This is in part based on the assumption that the younger students have better natural endowments than the older ones of the same class, since it has taken the older ones longer to reach the grade. This may be a safe criterion in some schools but usually the conditions which determine the age at which a pupil reaches a certain grade are so complicated that it is doubtful if it could be put to practical use. The article is interesting and suggestive and the results seem to justify the writer's conclusion that the Ebbinghaus method is of use for both normal and pathological subjects.

J. F. MESSENGER.

COLUMBIA UNIVERSITY.

PSYCHOLOGICAL REVIEW MONOGRAPHS.

The Practice Curve — A Study in the Formation of Habits. J. H. BAIR. Psychol. Rev., Mon. Sup., Vol. V., No. 2, Nov., 1902.

This research was occupied for the most part with an investigation of the conditions under which habits are formed and broken, and the laws governing them.

The method employed was to practice a series of stimuli, respectively, with a series of responses until the process became automatic, or until at a given rate no more errors were made. The instrument used was a typewriter, on the carriage of which was placed a series of colors which moved with it and of which one after the other was

exposed through a slot in a screen placed tight in front of the series. Colored caps corresponding to the series of colors were placed over several of the keys in a certain order. The experiment consisted in pressing down these keys as rapidly as possible as their corresponding colors appeared in the slot. The rate at which the series could be completed without making errors was recorded from time to time as the practices were continued until the maximum of speed was attained. The curve of progress for speed represents an asymptotic approach to a physiological limit. Another form of the experiment was when the rate of response was kept uniform by keeping time with the beat of the metronome, and rating the practice skill in terms of the number of errors made. Here again the same law is expressed in the progressive elimination of errors with succeeding practices. This experiment was made with different rates of the metronome and the results show that the more rapid the rate the greater the number of errors and the more practices required to eliminate them at that rate.

When the practice was long enough continued so that the series of responses had become thoroughly coördinated with the series of stimuli a change in the relation was made.

First, the order of the series of colors behind the screen was changed and the practice was continued and the increase in time required and the number of experiments to attain the old speed; or, the number of errors made and the number of practices to eliminate them were noted. *Second*, the order of responses was interrupted by reversing the order of keys and the time recorded as above. *Third*, both the series behind the screen and the order of the caps were interchanged and the results noted accordingly as above.

This experiment shows that the increase in time required and the number of practices to reduce it to the former speed is less when the series is changed than when the caps are changed, and most when both caps and series are changed, but in no case as much as in the first practice.

This experiment shows that the sensory associations, in the series, and the motor ones or less definite than those which are sensori-motor.

Further experiments were made to determine more definitely the relation between the number of practices made in one order and their practice effect, or interference effect, on the ability to run over a new series which is antagonistic to the one practiced. It was found that there is no such a thing as interference and that continued practice in one order increases proportionately the ability to make quickly and ac-

curately a new and antagonistic order. This law of the decrease of interference as we pass from one order to the other is also expressed by a curve similar to that described above, only the approach to the limit where there is no more interference experienced in changing from one to the other, is much more gradual.

Other experiments were made with cards which corroborate the same fact of learning and interference.

THE AUTHOR.

COLUMBIA UNIVERSITY.

Motor, Visual and Applied Rhythms. JAMES BURT MINER. Psychol. Rev., Monog. Sup., Vol. V., No. 21, 1903.

The thesis brings together four lines of investigation related to rhythm. A revision of the explanation of rhythm is attempted on the basis of muscle curves obtained during involuntary movement. A beginning is made in the study of rhythms experienced from flashes of light. The reproduction of time intervals is tested under different conditions. Finally, from the more practical side, the effect of independent rhythms on mental work is approached by correlation methods.

The first part of the paper deals mainly with the explanation of the feeling of unity in the rhythmic group. Curves are given which demonstrate, at least for eight subjects, that the motor effect of listening to a series of like sounds was something more than a single muscular response to each stimulus. The involuntary activity of the muscle seemed to be a reaction which set off succeeding groups of stimuli. A kymograph record is printed which shows that electrical stimuli applied successively to the thumb may produce this same regular grouping activity of the muscles. It seems, therefore, to be a fundamental structural condition similar to that producing the grouped reactions shown by the experiments of Richet, Lombard and others. When actual movements are not recorded the writer concludes that they are replaced by strain conditions in the muscle. The absence of movement is accounted for by the fact that most people in the waking state always hold their muscles sufficiently tense to prevent any slight tendency to movement. This conclusion is supported particularly by movement curves obtained, without suggestion, from a subject in the hypnotic state, although none were found normally. The involuntary grouping activity in the muscles is interpreted as the physiological correlate which explains the 'unitary character' of the rhythmic group. The tendency of experimenters to relate rhythm more or less definitely

to muscular activity is traced through the work of Bolton, Meyer, Wundt, Smith, Stetson, MacDougall and others. The author rejects, as inadequate or incomplete, those explanations which are based on regular bodily rhythms, on attention or on expectation and satisfaction.

If a kinæsthetic explanation holds, it would seem that visual rhythms, or even rhythms of odors and tastes, might be experienced. Contrary to general opinion, a subjective experience of grouping within a uniform series of light flashes was found to be quite easily developed by the twenty-six subjects tested. Two naïve subjects seemed to perceive involuntarily the rhythmic grouping, without having received any external suggestion. In the essential characteristics of rhythm, the effect of the lights was apparently the same as that of the sounds; but the light rhythm was more vague and easier disturbed. On account of visual rhythm being a novel experience, it offers suggestions toward the general problem that are obscured in the introspection of the familiar auditory rhythms. The connection of rhythmic changes with muscular activity comes out more plainly; illusions due to rhythmic causes are separable, by means of tabular classification, from those due to other conditions; finally, rhythms of sight are especially helpful in tracing the genesis from simpler forms — the investigation here corroborates Squire's conclusion that the unaccented group is the most primitive. A silent electric contact wheel was devised to be used with a relay and incandescent lamp, in throwing a flash against a wall. With this apparatus it was possible to compare the appearance of a uniform series with one in which the lights varied in interval, duration or intensity.

The third part of the monograph includes two series of experiments in the motor expression of time intervals. Under a chance arrangment, intervals of 1, 2, 3, 4 and 6 seconds were reproduced once after each occurrence of the standard. The reproduction was made by two taps on a telegraph key. Tables are given for the results of five subjects making 100 reproductions of each interval. A marked difference (amounting on an average to half a second) is demonstrated between the reproduction of the intervals when the standard is bounded by like stimuli (two lights or two sounds) as compared with the same intervals when the standard consists of a light followed by a sound or vice versa. This result is in conformity with the theory held by Münsterberg and others that the reproduction of a time interval consists of an attempt to repeat the strain sensations remembered. The muscular adjustment following stimuli directed to different sense organs requires more effort and makes the interval seem longer. The

change necessary in merely perceiving first a sound and then a light would hardly account for the pronounced lengthening which occurs. A new 'Carbon-Ribbon Kymograph' is shown. It utilizes the common typewriter ribbon in making records with electric pens on a telegraph-ticker tape. Records were obtained at Columbia University for 140 subjects making reproductions of a one-second interval continuously for 40 seconds. These records of their memory of a time interval were then correlated with the reaction times of the same subjects, using the Karl Pearson formula. A correlation of .55 brings out the interesting result that there is a strong tendency for slow reaction time to be found among those who shorten the interval most in reproducing it. This agrees with Seashore's suggestion that a brief interval probably seems shorter to the slow person than it does to the quick. The results are further discussed in connection with the sense of time and the theory of indifference points criticised.

Does an independent rhythm, which is kept up while we are working, hinder or aid us? In the last part of the thesis this question was answered, somewhat curiously, for groups of a hundred subjects, in respect to the effect of a metronome on a continuous choice reaction (distributing playing cards according to suits) and as to the effect of beating a rhythm with the fingers while filling words in blanks left in a poem. The correlations were .75 in the poem experiment and .32 and .39 in the choice experiment. These indicate an important principle which is suggestive for improving mental ability. Two classes of people are demonstrated, on whom the independent rhythm tends to have opposite effects. Those who were normally slow in the activities tested, tend to profit by the independent rhythmical stimulus. Those who were normally quick were seriously disturbed. The suggestion is made that the condition of very keen attention, found among the quick, is a more sensitive equilibrium which the secondary stimulus upsets. The slow person, on the contrary, is urged on by the accompanying rhythm.

THE AUTHOR.

COLUMBIA UNIVERSITY.

Sociality and Sympathy. J. W. L. JONES. Psychol. Rev., Mon. Sup., Vol. V., No. 1, April, 1903.

Sympathy is defined as the feeling accompanying a representation or memory state, when referred by the subject to an object. Sympathy in this view presupposes self-consciousness. It is involved in the conscious reference of a state of the subject to some object.

After comparing this definition with representative definitions of sympathy, the author analyzes what is presumably an instance of sympathy in order to show how the elements of the sympathetic state are contained in the definition given.

Imagination is not considered necessary to sympathy. I do not begin to sympathize by identifying myself with another. The sympathetic 'reference' is made, because, in the first place, the other person is identified with me. Imagination may, however, enter in as a factor of systematic or habitual sympathy.

The discussion of the definition is followed by an attempt to fix the rise of sympathy in the race and the individual. This is made by tracing out in the history of conscious experience, the causes that contribute to make a sympathy possible. The most fundamental factor in the state of consciousness immediately underlying a sympathy is the consciousness of kind, the recognition of oneself in an object. Genetically speaking, 'kind' means being of the same species, and the consciousness of kind signifies the consciousness involved in the various manifestations of the social relationship.

The social relationship is foreshadowed in 'passive association' (Spencer). The earliest form of sociality is the feeling of inclination, which creatures of a kind come to have for each other from simply living together. The social relationship proper arises out of organic imitation of one creature by another principally along the lines of defense. The defense may be by attitudes of flight with corresponding feelings of fear, or by resistance with corresponding feelings of anger. In the habitual attitudes of coöperative resistance to a hostile force is found the ground work of all social structure. At the earliest period they may be called collectively the instinct of mutual aid.

The rise of the sense of self and the representative consciousness with its memories and recognitions are important factors in the rise of the consciousness of kind. They do not, however, yield of themselves the consciousness of kind. For this there is required a situation in which one creature may become the object of another creature's deliberation in his own stead and not as a mere means to the other's safety or salvation. This situation is adumbrated in the phenomenon of play, and particularly in the restraints put upon play in the game. Especially in the restraints of play is found the way to conscious imitation of one creature by another, which at one and the same time develops the sense of self and gives rise to the consciousness of kind. Among the other factors that underlie sympathy, attention is called to the feeling of tenderness, which seems to be the sheer love of one's

kind, as such. Being like sympathy, a feeling experienced in repose, it is always a potent coadjutor in the expression of sympathy.

The instinct of mutual aid, having as its ulterior end the well-being of the individual is inimical to sympathy with its elements of vicarious self-sacrifice. For this reason sympathy springs up and reaches also its richest fruition in the family. Vicarious sympathy or altruism, entailing as it does reference to another, involves, just so far as it takes the individual apart from his own self-seeking, a consciously initiated variation upon the instinct of mutual aid. Hence altruism in the gregarious relationship is always an uncertain quantity. Some creature preëminently social and sympathetic may at times become a copy for imitations, which will tend to override the blind selfishness of the stereotyped social individual. At such times in the struggle for existence, the individual is conscious of acting in behalf of others as well as in his own behalf. Nevertheless such activity is not thoroughly purposive. Nor is altruism of value to society until moral personality furnishes standards that do justice to both the ego and alter elements in individual activity. It is in sympathy with such a moral ideal that the individual makes his altruism become a factor in social progress.

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BIOLOGICAL.

Variation in Animals and Plants. H. M. VERNON. New York, Holt. 1903. Pp. ix + 415.

This book is a thorough going-over of the question of variation in its many aspects by a competent hand. It shows the present state of the problem — notably its unsettled state, pending the further application of exact statistical methods. On the whole, Vernon finds that research to date upholds the Darwinian conception, both in the matter of the distribution of variations and in that of their cause. He is also Darwinian in his opinion as to use-inheritance, although impressed with the force of the point that in certain cases the effects of environmental changes seem to be cumulative from generation to generation. To account for this — which is, by the way, even if proved, not specific enough to comfort the Lamarkians — Vernon urges the possibility of such changes being due to modification of the internal secretions which are supposed to affect the germ cells of the organism more readily than would other modifications. As Vernon

himself notes, this suggestion had already been made by Delage. In the matter of the apparent operation of the use-inheritance principle, Vernon comes upon the idea — apparently independently — that has been expounded recently by others under the term 'organic selection'; and in this, too, he recognizes that the point had been made before by Lloyd Morgan, in 'Habit and Instinct'. On this point Vernon says: "Supposing for instance a number of organisms are more or less suddenly exposed to a considerable change of environment, whereby the majority of them are killed off. The survivors will be those which had the greatest power of adaptation [accommodation] to new surroundings, and though the somatic variations [modifications] will not be as such inherited, yet the survivors will be, on the whole, those organisms which originally possessed the largest proportion of the particular characters which have appeared as adaptive somatic variations * * * and hence the average hereditary characters of the survivors are in the direction of adaptation" (p. 390). This general solution of the determinate character of evolution is gaining ground steadily among biologists and psychologists and this opinion of Vernon will give to it further authority.

On the whole, readers of this book will find in it altogether the best general résumé on the topic of variation. The author also expounds certain original experimental results of his own. Certain points in the recently developed biometrical methods are given elementary statement for less technically educated readers. One of our reservations, however, is in the matter of terminology; it would seem to be a pity that Dr. Vernon does not adopt certain distinctions recently made in terminology, *e. g.*, that between variation and modification, that between adaptation and accommodation, that between development and evolution, etc.

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More Letters of Charles Darwin. FRANCIS DARWIN (editor), New York, Appletons. 1903. Pp. xxiv + 494, and viii + 508.

These two volumes, published in America uniformly with the Huxley letters and by the same house, are a most interesting edition to the literature of Darwinism. They are handsomely printed and illustrated with good portraits of many of Darwin's contemporaries.

The letters shed fine light upon the personality and personal relations of the great master of modern biology. Of especial interest

on this side the water are the letters to American biologists.[1] Asa Gray stands out in high relief (I., 69 f) in Darwin's estimate of the contemporary Americans. We have also in these letters frank allusions to the neo-vitalistic movement led by Cope and Hyatt, and a confession of failure to understand, made in the polite tone which gives point to the charge of essential mysticism (I., 341 ff).

Among the interesting side-lights thrown upon questions still under discussion the present writer notes the views of Darwin on 'saltation' or abrupt variation, and on inter-specific sterility. As to the former, it is well to remember that Darwin made extended inquiries as to the possible origin of strains breeding true from sport variation, and at the same times saw that such cases were rare and not available for a general theory of descent (see letters 95, 169, etc.). In the following words, written to Lyell in 1860 (letter 95), he anticipates a valid criticism against the extreme mutationists, *e. g.*, Scott. He says: "Harvey does not see that if only a few (as he supposes) of the seedlings being [are] monstrosities [abrupt variations], natural selection would be necessary to select and preserve them." These words would be subscribed to indeed by DeVries. Again he says, writing to Harvey (letter 110): "About sudden jumps: I have no objection to them — they would aid me in some cases. All I can say is that I went into the subject, and found no evidence to make me believe in jumps; and a good deal pointing in the other direction." And in a letter to Hooker (No. 135, 1862): "I am not shaken about *saltus*. I did not write without going pretty carefully into all the cases of normal structure in animals resembling monstrosities[2] which appear *per saltus*."

In the following remark, from letter No. 169 to Asa Gray, Darwin makes the point in regard to co-adaptations coming by chance in a single variation, which has been so strongly urged recently against natural selection in general (*e. g.*, by Romanes and others in the case of instinct; the further argument being that incomplete beginnings of such functions would be of no use); he says: "Do you not consider such cases as all the orchids next thing to a demonstration against Heer's view of species arising suddenly by monstrosities? — it is impossible to imagine so many co-adaptations being formed all by a chance blow. Of course, creationists would cut the enigma."

As to the sterility of species *inter se*, the fluctuations of opinion in

[1] In a recent issue of the *Pop. Sci. Monthly*, many of these American letters were given advance publication.

[2] Darwin used 'monstrosity' for all cases of extreme or 'sport' variation.

Darwin's mind clearly appear — notably the reiterated conviction that the subject was too obscure for a definite belief (see the most interesting letter 92, also 156, 157, all to Huxley). He often expresses the opinion that sterility may be eliminated by domestication, the exact adjustments of the two species to different environments (from which the sterility arose) no longer being maintained; and that it is for this reason that varieties produced by artificial selection — being practically domesticated — do not show the sterility *inter se* which natural species do. In one place however he makes a remark which, as the editors of the work point out (note to letter 153), anticipates Romanes' way (physiological selection) of accounting for such sterility by original variation; a suggestion also made by Darwin in a letter to Huxley printed in the 'Life and Letters,' II., p. 384.[1] He suggests experiments of the subject (letters 153, 154, etc.). In letter 157 he remarks (pursuing the idea about domestication mentioned above): "There must be something in domestication — perhaps the less stable conditions, the very cause which induces so much variability — which eliminates the natural sterility of species when crossed. If so, we can see how unlikely that sterility should arise between domestic races." The passage I cite, moreover, to suggest — what Darwin apparently does not mean — that just the increased variability which he notes in domesticated animals, and which would extend to the reproductive organs, may be the occasion of the absence of sterility — by giving more chances for the preservation of fertility under conditions of increasing divergence, under which in nature, with less variation, it would not be preserved. Possibly this suggestion has already been made by some one — it may be by Darwin himself elsewhere — but I have not met with it.[2]

Psychologists will be interested in Darwin's enthusiasm for child study, as shown in the following (letter 208, to Huxley): "Give

[1] The extended arguments of Darwin and Wallace given in letters 208–214 (especially Wallace's, No. 211) involve, indeed, a pretty complete statement of 'Physiological Selection.'

[2] The present writer has recently suggested that the progression in the teeth in fossils cited by paleontologists to prove determinate variation without natural selection (the early stages not being useful or not being in the direction of individual use), may have proceeded in correlation with muscular or other adaptive functions. Darwin makes about the same point (letter 143, to Falconer): "To explain how I imagine the teeth of your elephants change I should look at the change as indirectly resulting from changes in the form of the jaws, or from the development of tusks, or in the case of the *primigenius* even from correlation with the woolly coverings; in all cases natural selection checking the variation."

Mrs. Huxley the enclosed [queries on expression], and ask her to look out when one of her children is struggling and just going to burst out crying. A dear young lady near here plagued a very young child for my sake till it cried and I saw the eyebrows, for a second or two, beautifully oblique just before the torrent of tears began."

Other matters worth noting are (and many besides might be written down) the opinions of John Scott, Darwin, and Huxley (letter 151, with the editors' notes), on the idea embodied in Weismann's theory of amphimixia (variability due to union of different sexual elements), in connection with which the need of the conservation of the mean, later formulated in Galton's principle of regression, seems to have impressed Darwin. "I see in the *Cornhill Magazine*, a notice of a work by Cohn on the contractile tissues of plants. * * * I am much interested in the subject and experimented a little on it this summer, and came to the conclusion that plants must contain some substance most closely analogous to the supposed diffused nervous matter in the lower animals."

Darwin's relation to Wallace is also brought out further in certain of the letters. We read of another incident in which the large-heartedness and generosity of Darwin appears in the matter of their common researches on sexual selection (II., 59 ff). Darwin's conscience seemed to have given him no peace after he had quite naturally suggested that Wallace was anticipating him; and he insisted on making amends and asking forgiveness. On the other side, Wallace is noble too; he offered to give his material to Darwin — a nobility shown so handsomely in his giving to his own book, expounding his own statement of natural selection, the title *Darwinism*. The relation of the two men is in all respects perhaps the finest lesson in the ethics of scientific discovery which was ever acted out; and how mean it is to cite Charles Darwin as a case of the atrophy of the finer sentiments on the basis of the passage, so much quoted, in which by his very self-depreciation, he is but illustrating that personal humility which is one of the finest of these sentiments! He may not have cared to read poetry, but he had that spiritual vision of the higher values of life which 'poets' so often degrade in practise.

These volumes should be put reverently on the library shelf within easy reach, along with the Huxley *Life and Letters*, by every one who loves honesty allied with strength.

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NEW BOOKS.

Le Goût. L. MARCHAND. Bibl. de Psych. expér. Paris, Doin. 1903. Pp. 332. 4 fr.

Studies in the Evolution of Industrial Society. R. T. ELY. The Citizen's Library, New York and London, Macmillans. 1903. Pp. xviii + 497. (An extremely interesting series of discussions, characterized by broad philosophical points of view.)—J. M. B.

Essai sur la Psycho-Physiologie des Monstres humains: Un anencéphale — un xiphopage. N. VASCHIDE and CL. URPAS. Paris, Rudeval. 1903. Pp. 294.

Studien zur Methodenlehre und Erkenntnisskritik. FRIEDRICH DREYER. Leipzig, Engelmann. Bd. I. 1895. Pp. xiii + 222. Bd. II. 1903. Pp. xxi + 498.

Etude expérimentale de l'Intelligence. A. BINET. Paris, Schleicher. 1903. Pp. 309. 6 fr.

Un Médicin philosophe au XVI^e^ Siècle (Jean Fernel). L. FIGARD. Paris, Alcan. 1903. Pp. 365. 7 fr. 50.

Vers le positivisme absolu par l'idealisme. LOUIS WEBER. Paris, Alcan. 1903. Pp. 396. 7 fr. 50.

L'Année philosophique. 13^e^ Année (1902). F. PILLON. Paris, Alcan. 1903. Pp. 306. 5 fr.

Saggio di uno Studio sui Sentimenti Morali. G. SALVADORI. Florence, Lumachi. 1903. Pp. 138.

Ueber die Bedeutung des Darwin'schen Selectionsprincips und Probleme der Artbildung. L. PLATE. 2te Auf. Leipzig, Engelmann. 1903. Pp. viii + 247. 5 M.

Sexual Dimorphism in the Animal Kingdon. J. T. CUNNINGHAM. London, Black; New York, Macmillans. 1900. Pp. xi + 317.

Études de Psychologie physiologique et pathologique. E. GLEY. Paris, Alcan. 1903. Pp. viii + 335. 5 fr. (A collection of papers including M. Gley's well-known researches on the 'muscular sense' and 'unconscious movements.')

Swain School Lectures. A. INGRAHAM. Chicago, Open Court Publishing Company. 1903. Pp. 197. $1.

Mental Traits of Sex. HELEN B. THOMPSON. Chicago, University of Chicago Press. 1903. Pp. vii + 188.

Lehrbuch der Wädschenerziehung. MARIE MARTIN. Leipzig, Dürrschen Buchhandlung. 1903. Pp. viii + 188.

NOTES.

WE are glad to learn that Professor William James' 'Varieties of Religious Experience' is being translated into French.

DR. ALBERT LEFEVRE has been appointed to the chair in Philosophy in Tulane University.

La Revue de Philosophie announces the issue of an *Annuaire des Philosophes* — devoted to men and things philosophical — under the charge of M. N. Vaschide, the energetic *Chef de Traveaux* of the Hautes-Études psychological laboratory. The *Revue* has issued a 'questionnaire' to philosophers, asking for various personal and professional details. As the success of such a publication depends upon its completeness we bespeak a courteous response to this circular; if revised at intervals it would constitute an interesting *Vade mecum.* (Address M. N. Vaschide, 56, rue Notre-Dame-des-Champs, Paris.)

IT is announced that the Fourth International Congress of Psychology is to meet at Rome in the spring of 1905 instead of the autumn of 1904, since the latter date would cause conflict with the Congress of Physiologists at Brussels.

WE also have to announce that the second International Congress of Philosophy is to be held at Geneva, in the first week of September, 1904. It is possibly not generally known that the scientific congresses of the St. Louis Exposition which are also to meet in the early autumn of 1904, will include all the mental, moral, and philosophical disciplines. It is to be hoped that the committees will arrange some sort of coöperation between the St. Louis and the Geneva Congresses.

WE regret to announce the death of M. Ernest Murisier, Professor of Philosophy at Neuchâtel.

WE have received the announcement of a projected *Journal de Psychologie pour la France et pour l'E'tranger*, to be edited by MM.

Pierre Janet and G. Dumas, and published by M. Felix Alcan, Paris (first No. Dec., 1903). It is to be issued bimonthly (about 144 pp.), and is to cover the whole field of psychology, giving full accounts of the literature.

THE Scribners announced that the following volumes have already been arranged for in Professor Baldwin's 'Library of Historical Psychology': 'Memory and Imagination,' by Professor G. F. Stout, (St. Andrews); 'Logical Processes,' by Professor John Dewey, (Chicago); 'Self-Consciousness and Personality,' by Professor Josiah Royce (Harvard); 'Sensation,' by C. Ladd Franklin (Baltimore); 'Æsthetics,' by Professor J. H. Tufts (Chicago); 'Epochs and Problems on the History of Psychology,' by the Editor. Other volumes are to be announced shortly.

IT is also proposed to issue in the same historical series a volume of important psychological texts — reprints of papers which are important to preserve and which have become rare or inaccessible. The original languages will be preserved except, possibly, in certain cases. The editor of the series requests suggestions, from any one interested, as to texts which it would be well to include (address J. Mark Baldwin, Princeton, N. J.).